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THE PLACE OF RESEARCH IN EDUCATION.[1]

THE address on Art Tuition delivered here a fortnight ago by Professor Herkomer—which I trust many of you had the advantage of listening to—was full of wise counsel, which cannot fail to be of value to those who study it; the more so as Professor Herkomer is not only himself an artist of wide and varied experience, highly gifted with originality, but also an experienced teacher, and is therefore better able to advise than are most other artists. For, after all, only the competent teacher is fully aware of the difficulties which beset the path of the student.

But no advice given by Professor Herkomer was equal in importance to his opening statement—which was subsequently confirmed by members of the governing body

[1] The editor has been good enough to regard the following article as of sufficient interest to warrant its insertion, distinctly with the object of showing that it is desired to deal with subjects likely to attract public attention, so as to interest a wider circle of readers in "SCIENCE PROGRESS". I have gladly accepted his hospitality, as I believe it to be of the utmost importance to interest scientific workers as well as the public in questions such as I raise. Unless *we* are prepared to throw ourselves more into such work, there is little probability that the teaching of scientific method will gain that popular recognition which the subject imperatively demands, and which so many of us are persuaded must without delay be accorded to it, if we are to retain any proper measure of national prosperity. It was recently delivered as an address at the Chelsea Polytechnic, and hence it is somewhat egotistical and dogmatic in style; however, I have thought it undesirable to change the style, desiring to lay as much emphasis as possible on the important issues which are considered.

—that in this Polytechnic *a clean beginning has been made in Art;* that you have advisedly elected to be free from all external control and are possessed with the fixed intention of working out your own salvation. Professor Herkomer begged—prayed I may say—that you should be kept clear of all contagion, and all who are your true friends must join in this prayer.

I desire to preach from the same parable as regards the teaching of Science—to exercise the functions of a Medical Officer of Health for Science; but my task is a difficult one. Professor Herkomer spoke to willing ears; his meaning was clear; he was dealing with a popular subject of which we all have some understanding. I cannot but recognise that my subject is generally misunderstood, and its public importance greatly underrated in consequence. Some inscrutable influence has led those who have organised this Institute to appreciate the needs of Art, and having had the wisdom to take proper advice, they have put you in possession of the elements of a perfect system of art sanitation; but the needs of the sister subject Science have yet to be grasped here and elsewhere. The sanitary condition of the dwellings which Science has to put up with throughout our country is most faulty, ill-arranged, out of date, and oftentimes abominable; and if this Polytechnic, indeed Polytechnics and schools generally, desire to place the teaching of Science under healthy conditions, heed must be given to the inspector's warnings.

I might almost take Professor Herkomer's address, write science for art, add a few passages here and there, and redeliver it as my own. Decorative art, that art which enables artists to decorate, you were told cannot be taught on the large scale, it cannot even be taught in schools—it must be taught in the workshop. Decorative science, science which decorates its possessor and enables him or her to be scientific, scientific knowledge which can be made use of in the service of the world, also cannot be studied except in the workshop and in Nature, to whom also the artist must resort. As Professor Herkomer said most truly: all technical education will fail if established on a

scheme in which the master's personality is eliminated, and that must follow in any scheme of wholesale tuition.

But what meaning have the words science and scientific in English ears generally? Do they excite visions of a complicated picture of things concerning our daily life in its minutest details? Certainly not! Their utterance before those who know a little chemistry recalls fireworks and smells and perhaps simple salts; whilst those who take an interest in electricity have thoughts of bells ringing, galvanometer needles wagging, or glowing electric lights; and teachers dream of South Kensington certificates and hardly earnt grants. In the minds of the general public they call forth no response, especially in those of that very numerous section of the community which is concerned in commercial transactions and has no knowledge of manufactures. Science in the eyes of the average Englishman consists of a new-fangled set of ideas, all very well for those who can afford the time to study them, but in his opinion not of such daily practical importance that it is necessary for the nation to pay attention to them. And this unfortunately is the opinion even of "educated" men and of many men of culture. This is perhaps the primary defect in our system to which the Medical Officer of Health for Science is bound to call attention; it is one which we must all unite in overcoming and which Polytechnics such as this should do much to remove.

If public appreciation of scientific procedure can be secured to even a moderate extent, a complete popular victory for those who press for its introduction must soon follow; the advantages to be derived from the general application of scientific method to the affairs of life are demonstrably so great that when once they are made known at all commonly its adoption will be insisted on.

Science is but exact knowledge, and there are as many branches of science as there are of exact knowledge. Remember, however, a loose incoherent body of facts does not constitute a science—a man who is merely possessed of such facts is not scientifically trained. A scientific man is a "knowing man"—not merely a man who knows, but one

who is properly described in the terms of the popular expression—He's a knowing fellow—which implies something more than the mere possession of knowledge, namely, the power to use it properly and with effect. There is every difference, in fact, between the scientific and the merely learned man. To be scientific is to be as far as possible exact in thought, deed, and word; to act with a purpose and after due and careful consideration; to be observant and thoughtful; to be logical and methodical; to be guarded but fearless in opinions and judgment: and it is because we are so rarely all these that we are so rarely truly scientific.

Unfortunately the word science is now associated in the popular mind with certain branches of natural knowledge, and it is because these are generally regarded as of importance only to those whose special business it is to attend to them that the proper application of the term is lost sight of.

I am not here to speak of science teaching—I do not know what that is—but of *scientific* teaching; of the method of teaching scientifically, that is to say, exactly and properly. I am really speaking on the very subject on which Professor Herkomer dilated; we are both pleading one cause although on behalf of somewhat different interests, and mine is the wider plea, and will, in fact, include his. He was the advocate of a practical workshop method of art tuition, under a teacher free as well as competent to consider the peculiar qualities and requirements of his pupils; of a method of so training students as to develop to the utmost their individual innate talents, instead of turning out a set of mechanical automata, blind followers of fashion. I desire to urge that whatever we teach, our method shall be scientific, so that students, in proportion to their abilities, may learn to honestly and usefully apply whatever knowledge they may become possessed of.

Institutions such as this have a great field of usefulness before them if all their work be done from such a standpoint; if it be not they will be absolute and costly failures. I much fear that unless a change in policy, almost amounting to a revolution, take place in many of the schools throughout

the country, devoted to what we are now pleased to call technical education, the results will be disastrous.

Let us consider what has been and is being done. Until about twenty years ago, besides our Universities, the three London Colleges and Owens College, Manchester, the country had little to boast of in the way of institutions for higher learning to which those who had left school could resort; but then important University Colleges were founded in rapid succession in a number of the chief provincial towns. Meanwhile the educational fever spread to London, and there assumed an extremely acute form under the name of Technical Education. The City and Guilds of London Institute was founded, and built first the Finsbury Technical College, and later the Central Technical College at South Kensington, besides establishing an Art School at Lambeth; and by taking over, fostering and largely extending the system of Technological Examinations initiated by the Society of Arts, the City Guilds Institute exercised an extraordinary influence on the establishment and conduct of evening classes for instruction in technical subjects throughout the country. A further development of the same spirit has led more recently to the erection here, there and everywhere throughout London of Polytechnics, etc., and of a large number of technical schools of various degrees of importance in the provincial towns.

Why, it may be asked, all this educational activity, and why especially did the cause of technical education so suddenly spring into prominence? The answer is, you know, because the conviction arose that our manufacturing industries were being seriously threatened in consequence of our failure to sufficiently avail ourselves of scientific aid, and the greater appreciation by foreigners of the services of scientifically trained workers. Because a feeling was abroad such as was graphically expressed by Huxley in a remarkable letter to the *Times* at the close of 1886 in which he pointed out that we had "already entered upon the most serious struggle for existence to which this country has ever been committed," adding "the latter years of this century promise to see us embarked in an industrial war

of far more serious import than the military wars of its opening years. On the East, the most systematically instructed and best-informed people in Europe are our competitors; on the West, an energetic offshoot of our own stock, grown bigger than its parent, enters upon the struggle, possessed of natural resources to which we can make no pretension, and with every prospect of soon possessing that cheap labour by which they may be effectually utilised. Many circumstances tend to justify the hope that we may hold our own if we are careful to organise victory."

The question is—have the steps we have taken to protect ourselves, to hold our own, to organise victory, led to success? In most cases, most certainly not! We are fast proving ourselves to be incapable of holding our own in almost every branch of industry.

Of course there are certain brilliant exceptions to the general rule, but these only prove the rule and enable us to understand the cause of our failure.

Why is this? It is, I believe, because our character is so firmly set that nothing but severe compulsion will lead us to reform; it is because, whatever we may term ourselves politically, we are all by nature ultra-conservative, the rankest political radicals amongst us being the strongest conservatives in their general conduct. It is due to our intense belief in ourselves, the outcome of a long period of unexampled prosperity. We are so intolerantly individual that we cannot bring ourselves to organise and co-operate, and this probably is the main source from which our difficulties spring. Let me take an illustration from agriculture, the most important of our industries, but one which as all know is in a terribly depressed condition. We are told that last year £36,000,000 worth of butter, cheese, eggs, hams, bacon, fowls, ducks, etc., was *imported* into this country! Surely we ought to be capable of producing most of these.

As a matter of fact we cannot even make decent butter or cheese yet. As Sir Henry Gilbert, the distinguished agricultural chemist, remarked to me lately when we were sitting together at dinner: "In our village we prefer to buy Brittany butter rather than the local 'Best Fresh'". If we could

do such things, it would not be necessary for the Yorkshire College, our leading University College, to send out peripatetic teachers to instruct dairymaids, or for County Councils all over the country to do similar work, nor would the Duke of Devonshire have been called on as he was a few weeks ago to open a Midland Dairy Institute. In France they have long known, not only how to make butter properly, but what to do with it when they have made it, an important art which we, from our inability to organise, have also yet to learn. Let me recite my own experience in this matter.

My father was largely concerned with the Normandy butter trade, and on one occasion, about thirty years ago now, I visited France with him; nothing I have since seen has ever impressed me more. The morning after our arrival we were driven out to a market town in the district, where we found all the country folk collected together, each having brought whatever produce they could command to market; our friend, and I should say that the active worker was a woman, went rapidly round the market, tasting each parcel of butter and offering what was thought to be its value, which, of course, was not the retail price of best fresh. If the bid was accepted note was taken by a clerk. On our return, after dinner, in the evening, we found that the butter had not only been collected and brought in, but we actually saw it being carefully mixed and salted and coloured to standard, so as to make it all of one uniform quality, enough to fill a large number of casks being thus dealt with; the next morning it was on the rail and on its way to England. No such thing, I believe, has ever yet been done in this country.

As to eggs coming from abroad, a recent remark made by Sir J. B. Lawes occurs to me: "that it is not that we do not produce them, but we eat them nearly all ourselves". It may be well for us that we do, but the fact is none the less an illustration of the absence from England of thrifty habits such as characterise other nations.

Again, to illustrate why we are beaten by others, let me refer to the fate that has befallen what was formerly an

important English industry—the manufacture of colours from coal-tar, which is now practically in the hands of the Germans and Swiss, so much so that Dr. Caro, the chief living authority on these matters, in addressing the German Chemical Society a couple of years ago was able to refer to it as a German national industry.

It was established in 1856 at Sudbury, near Harrow, by Perkin, who discovered the first aniline colour in the course of a research which he was carrying out, with purely scientific objects in view, under the direction of Hofmann, then Professor in the Royal College of Chemistry, in Oxford Street, London. Soon afterwards the important firm of Simpson, Maull & Nicholson was founded at Hackney Wick—Nicholson being another of Hofmann's pupils. Although similar works were erected in France and Germany, the main business remained in English hands during perhaps twenty years. Meanwhile Dr. Griess—chemist throughout his life to the celebrated brewers at Burton-on-Trent, Messrs. Allsopp—was carrying on researches on diazo-compounds, which he had begun as a student in Germany—one of the most remarkable series of scientific researches ever made; but these did not meet with full appreciation until 1876. In this year the firm of Williams, Thomas & Dower of Brentford introduced certain azo-colours into the market which had been made in their works under the direction of a most accomplished Swiss chemist, Dr. O. N. Witt, strictly in accordance with Griess's prescriptions. The importance of the step thus taken was not fully apparent here, but it was in Germany. Dr. Caro, a member of the now world-renowned Badische Anilin und Soda Fabrik, near Mannheim on the Rhine, who had formerly been chemist to Roberts, Dale & Co., in Manchester—the personal friend of Griess—had been working in the same direction as Witt, and his firm shortly afterwards brought out azo-colours similar to those manufactured by the English firm. This time the seed had fallen upon fruitful soil: the Germans were theoretical as well as practical, and at once saw that the application of Griess's discoveries was likely to be productive of practical con-

sequences. They largely increased their scientific staff—research became the business of the works, and the industry expanded at an extraordinary rate, while the English manufacturers, remaining unteachable, and having no proper scientific staff in their employ, were simply snuffed out.

And the story has yet another side—you have all heard of the turkey-red or madder dyes, formerly obtained from the madder plant which was very largely grown in France, Holland and Turkey. In 1868 two German chemists prepared alizarin, which is the chief constituent of madder, artificially from anthracene—a substance contained in coal-tar. Now Perkin, when a student with Hofmann, had worked with anthracene, and seeing the practical importance of the discovery again set to work and anticipated Graebe and Liebermann in the discovery of a process of manufacturing alizarin. He at once began to make it artificially—his works prospered and during several years the production of artificial alizarin was an English industry. But Perkin made the unfortunate "English" mistake of working almost single-handed — the Germans, meanwhile, were silently but steadily working in their characteristic manner, studying every detail, and soon came to the fore and became masters of the situation.

Perkin's business is now continued as the British Alizarin Company. The conditions under which this firm is working are somewhat peculiar, and such as to procure for it considerable advantages, but the success which has attended its labours is sufficient to show that such an industry might be carried on with special advantage in this country if organised in the proper spirit. Several years ago I had the opportunity of visiting the works shortly after I had inspected the most fully equipped factory of the kind in Germany, and I was agreeably surprised to find that the English works were distinctly in advance of their continental competitors, being able to deal economically with larger quantities. But whereas here the anthracene colour industry is much as it was, abroad it has expanded in various important directions which are proving highly

remunerative, whilst the original madder dyes, although produced in larger quantity than ever, are made at slight profit owing to the excessive competition that has arisen.

Now artificially made dyes have all but displaced natural colouring matters, indigo excepted, even among so conservative a people as our Indian subjects, and the industry is of enormous importance, although not to us. We are so much behindhand in the race that there is little chance of our regaining a good place in the list of runners, even if we go fully into training with that object.

And not only dye stuffs are made from coal-tar. A whole list of substances of the greatest value in medicine are also now prepared from raw materials derived from tar; some of these have proved to be most efficient substitutes for quinine, and the growth of cinchona bark in India and Ceylon has consequently ceased to be the remunerative pursuit it was. All such substances have been the outcome of researches carried out in the German Universities, or in the still more highly equipped laboratories of the German chemical works. Moreover, of late years, Nature's perfumes have one after the other been forced to disclose their character to the pertinacious inquirer, and have been claimed as victims by the chemical manufacturer—abroad, although here again the example was first set by Perkin, who in 1868 showed how *Coumarin*, the odoriferous principle of the Tonka bean, might be artificially prepared.

If we seek to understand our early success as well as our later failure in the branch of industry of which I have been speaking, it is not difficult to trace the former to Hofmann's influence and the latter to our want of appreciation of the inestimable value of the services of such a man. In this connection I may be allowed to quote from my Presidential Address to the Chemical Society in 1894 the following passage in reference to the then recently published memoir by Dr. Caro on the development of the coal-tar colour industry:—

"To those who can understand it, the story told by Caro is nothing less than an epic, but it is one the contemplation of which must in many ways sadden an Englishman,

elevating though it be when regarded from the purely scientific point of view. Full and complete recognition of Hofmann's services to the industry characterises every page of the monograph, and the only ground of complaint which some of us feel that we have against the writer is that his own great services are nowhere referred to. Wherever they received their early training, the true education which experience in the world alone gives was gained by both Hofmann and Caro while in the service of English masters; and it is an interesting problem for speculation whether, had they remained with us, our position would not have been a different one. Germany could scarcely have accomplished what it has done without them, but by years of patient labour her Universities had laid a broad and solid foundation on which alone such men could build. Here, such men had neither brick nor mortar offered to them either by the Universities or manufacturers, and such is our disregard of theory in this country of 'practical men,' that we even now have not learnt the lesson which the contemplation of the success of German chemical industry teaches; shall we ever learn it properly? In London, at all events, we shall probably wrangle during years to come about the establishment of a University worthy of the greatest city in the world, which will set an example and help us again to do our fair share of the work which has been taken from us; and it will be years, apparently, before English manufacturers will all learn to spell the word chemist—and that it will acquire some meaning for them. But it is much to be feared that recantation may come too late, and that the opportunity will have been lost. America, perhaps, will meanwhile have learnt the lesson also, and the competition we shall have to meet will not be European alone; we have not only to go ahead as fast as others, but to make up for much lost time, and it is not likely that others will calmly stand by while we make the attempt.

"Such is the lesson which we may derive, it seems to me, from the study of Hofmann's career and the attendant circumstances; and it is one which we in this Society must take very deeply to heart."

At the present day, no matter what his business, the German manufacturer seeks to understand every detail, and he is always trying to improve his processes. He attains this end by availing himself very fully of the services of men trained *scientifically* at the University, men who have all served their apprenticeship in the school of research ; in fact no man who has not been so trained is looked at now-a-days by the German manufacturer.

In proof of this I may quote words used by Dr. O. N. Witt, now Professor of Chemical Technology in the Berlin Royal Technical High School, the most important institution of its kind in the world, in his report to the German Government as their commissioner at the Chicago Exhibition in 1893. Says Dr. Witt : "What appears to me to be of far greater importance to German chemical industry than its predominant appearance at the Columbian World's Show is the fact which finds expression in the German exhibits alone that industry and science stand on the footing of mutual deepest appreciation, one ever influencing the other; by affording proof that this is truly the case, Germany has given an indisputable guarantee of the vitality of its chemical industries".

Our policy is the precise reverse of that followed in Germany. Our manufacturers generally do not know what the word "research" means ; they place their business under the control of practical men, often admirable men in their way, possessed of much native wit, but untrained and therefore too often and necessarily unprogressive ; and such men as a rule actually resent the introduction into the works of scientifically trained assistants. Hence there is no demand here for men who have been carefully trained as investigators ; consequently our schools do not seriously attempt to train investigators ; in this country such people are only born and grow spontaneously, the high-class manufactured article is made in Germany alone. We elect to sacrifice at the altars of the examination Fiend, for God he cannot be called, and do our best to discourage the development of originality.

Let me give an illustration to make my meaning clearer.

Recently I met a friend who has not only distinguished himself by his intelligent criticism of a particular industry, but has become so interested in it that, having means at his disposal, he has himself become a manufacturer, affording a rare illustration of enterprise. I said: "I trust you are going to work on German lines and engage a good chemist to systematically study your material, and so ascertain how its properties vary with its composition; for I have reason to think from direct experience that much is to be learnt in this way which will make it possible to put the manufacture on a scientific basis". His ready answer was: "Oh, I've got to make the business a commercial success!" Of course I understood what he meant whilst I felt that he could not fathom my meaning—he was too much an Englishman to do that. No doubt he will place his business in the sole charge of a practical man, and as long as it suffices to look only at the surface he will succeed; but then, not improbably, the Japanese will come in and beat him, for they have shown the world that they can organise as well as appreciate scientific method.

Or to give another example showing what may be accomplished under English conditions by adopting foreign methods, let me refer to work done by Mr. Mond, so well known in this country on account of the skill he has shown in developing Solvay's ammonia-soda process. Mr. Mond has long been engaged in seeking for a solution of the problem—how to burn fuel electrically, in such a manner, that is to say, as to directly produce electricity instead of heat. Having improved the gas battery devised in 1842 by the present Sir William Grove, in which hydrogen is burnt electrically, he was anxious to obtain a method of preparing hydrogen readily in large quantities. No good method is known, but a mixture of hydrogen and carbonic oxide is easily made, and even Mond found that on passing this mixture over heated nickel the carbonic oxide was converted partly into carbon and partly into carbondioxide, and as the latter was easily removable he thus succeeded in a measure in effecting his object. In studying the very remarkable action which nickel had on carbonic oxide, it so

happened that on one occasion when experiments were being made in his laboratory the escaping gas was led into the flame of a burner so as to set fire to it, a necessary precaution as the gas is highly poisonous; it was noticed that instead of burning as usual with a non-luminous smokeless flame it burnt with a slightly luminous flame. This strange circumstance led to inquiry being made, and it was eventually ascertained that the metal nickel under certain conditions combined with the gas carbonic oxide, forming a very volatile colourless liquid, and thus one of the most remarkable discoveries of modern times was made. The discovery was communicated to the Chemical Society in 1890 by Mr. Mond in conjunction with his assistants, Drs. Langer and Quincke. Having observed that the compound was very readily broken up into carbonic oxide and nickel, Mr. Mond at once set to work to devise a practical method of preparing nickel on the large scale from its ores through the agency of the new compound, and after spending not only much time and labour, and I believe also a very great deal of money on his quest, was successful in devising a process which he has carried out on the large scale during several months past, and which has enabled him to produce over a ton of metallic nickel of almost absolute purity per week—perhaps the greatest achievement in metallurgy on record. Such action on the part of a native-born English manufacturer is "unthinkable," at least I know of no precedent which would justify us in regarding it as possible under present conditions. I only recently heard of a firm who are doing work of a most important and critical character, involving the expenditure of a very large amount of money, who, having asked an expert whether it would not be well to carefully observe the temperature at which their operations were conducted, on being advised that it was most important to do so, objected that an instrument for the purpose, costing £25, was too expensive to use. The foreign worker would seek to know what happens at any cost.

If the English nation is to do even its fair share of the work of the world in the future, its attitude must be entirely changed—it must realise that steam and electricity have

brought about a complete revolution, that the application of scientific principles and methods is becoming so universal elsewhere, that all here who wish to succeed must adopt them and therefore understand them. It rests with our schools to make the change possible. To repeat what I said in 1894 in the presidential address before referred to: "There can be no question that the future of this country is very largely in the hands of its schoolmasters and schoolmistresses, and there are many among us who are convinced that our progress is much hindered by their failure to feel the pulse of the times—who think, indeed, that they do not suitably prepare the material which we subsequently are called on to mould into its final shape. We look to the new Commission to recommend drastic changes which will enable us to utilise to the full the marvellous ability latent in the English race, and which will help parents in solving the truly terrible problem with which they are confronted in these days of unreasoning and unreasonable competition when the time comes to secure a career for their children. English boys and girls at the present day are the victims of excessive lesson learning, and are also falling a prey, in increasing numbers year by year, to the examination demon, which threatens to become by far the most ruthless monster the world has ever known either in fact or in fable. Ask any teacher who has to do with students fresh from school his opinion of them: he will say that in the great majority of cases they have little if any power of helping themselves, little desire to learn about things, little if any observing power, little desire to reason on what they see or are called on to witness; that they are destitute of the sense of accuracy, and satisfied with any performance however slovenly; that, in short, they are neither inquisitive nor acquisitive, and as they too often are idle as well, the opportunities offered to them are blindly sacrificed. A considerable proportion undoubtedly are by nature mentally very feeble; but the larger number are by no means without ability, and are, in fact, victims of an acquired disease. We *must* find a remedy for this state of things, or perish in the face of the terrific competition now setting in. Boys

and girls at school must be taught from the very earliest moment to *do* and to *appreciate*. It is of no use our teaching them merely *about* things, however interesting—no facts must be taught *without their use* being taught simultaneously; and, as far as possible, they must be led to discover the facts for themselves. Instead of our placing condensed summaries in their hands, we must lead them to use works of reference and acquire the habit of finding out; they must always be at work applying their knowledge and solving problems. It is a libel on the human race to say, as many do, that children cannot think and reason, and that they can only be taught facts; early childhood is the time at which these faculties are most apparent, and it is probably through failure to exercise them then that they suffer atrophy. The so-called science introduced into a few schools in answer to the persistent demands of its advocates has been in most cases a shallow fraud, of no value whatever educationally. Boys see oxygen made and things burnt in it, which gives them much pleasure; but, after all, this is but the old lesson learning in an interesting shape, and has no superior *educational* effect. I would here repeat what I have recently urged elsewhere, that in the future *all subjects* must be taught *scientifically* at schools, in order to inculcate those habits of mind which are termed scientific habits; the teaching of *scientific method*—not the mere shibboleths of some branch of natural science—must be insisted on. No doubt some branch of chemistry, with a due modicum of physics, etc., is the subject by means of which we may, in the first instance, best instil the scientific habits associated with experimental studies, but it must be the true chemistry of the discoverer, not the cookery-book-receipt pseudoform which has so long usurped its place. Whatever be taught, let me repeat that mere repetition work and lesson learning *must* give place to a system of allowing children to *do* things themselves. Should we succeed in infusing the research spirit into our teaching generally, then there will be hope that, in the course of a generation or so, we shall cease to be the Philistines we are at the present time; the education given in our schools will be worthy of being

named a '*liberal education*,' which it never will be so long as we worship the old-world classical fetish, and allow our schools to be controlled by those who reverence this alone, having never been instructed in a wider faith."

And what are schools such as this to do? Should they not also teach in the same spirit? As the Secondary Education Commissioners point out: Education must ever become more practical—a means of forming men (and they should have added, and women) not simply to enjoy life but to accomplish something in the life they enjoy.

To this end, every school, I believe, whether in this metropolis or elsewhere, must work out its own salvation; and we must not look for payment on results, or countenance examinations which reduce all to one dead level.

When Professor Ayrton and I were appointed the first professors of the City and Guilds of London Institute—he having cut his educational teeth in the service of the Japanese and I having been largely made in Germany—we found ourselves in complete agreement that we would have nothing to do with teaching for examinations. Those who afterwards became our colleagues in the establishment of the Finsbury Technical College, my friends Mr. (now Sir Philip) Magnus and Professor Perry fully shared this view, and we all saw that a big problem in education lay before us which we could only work out if we had complete liberty of action, and the Committees we had to do with never for one moment questioned this—all honour to them. I am proud to say that the programmes of the Guilds' Colleges have never been disfigured by references to examinations as objects to be kept in view by students, and I venture to think that when the time comes to consider without prejudice the services which the City Guilds have rendered to the cause of education, it will be admitted that they, more than any other body, have shown true appreciation of English needs. It is worth while noting that although it has never been a coaching college, the Finsbury Technical College has always been overfull, which disposes of the assertion that the bait of an examination must necessarily be held out as an attraction.

But what have the Polytechnics done? To what extent have they made *a clean beginning* in all subjects; and to what extent have they been suborned to worship at the examination shrine to earn the unholy money bribe called payment on results?

I am told that the latter course is adopted in some cases, not because it is felt to be the right one, but because it would not do for Polytechnic B to appear behind Polytechnic A in regard to the number of certificates gained—Governors might object! Unfortunately we know that such arguments are held, that quantity counts for more than quality. The English manufacturer can appreciate a big order, but will not undertake to carry out a small one; and here the foreigner steps in and having made a beginning gradually improves his position until finally he is left in practical possession of the field. Perhaps if we attended more to quality in education there would soon be a large increase in its quantity. A large proportion of those who at present come forward to be prepared belong to Professor Herkomer's great class of those who ought not to be taught at all. Among these are the certificate hunters brought into existence by School Boards and other authorities. There is more than sufficient work to be done among those who are deserving and capable.

To quote Professor Herkomer: "No system could act more perniciously on the morals than payment on results. A few schools have through their strong masters, and in some cases, strong local help, shaken off all the trammels of danger. The fact is, it has all grown into an unwieldy piece of machinery with all the deadening effect of *impersonality* in the teaching. The whole system, when it is not practically upset by a strong and independent master, is lifeless, humdrum, and above all soul-deadening. It is the despair of the masters and the disappointment of the brighter pupils." To all which, I say, Amen!

The system was established at a time when the many schools to which I have referred were unknown, and it was largely because the desired result was not obtained under the system that these new schools became necessary and

were founded. How futile then must be any attempt to base the instruction in these new institutions on discredited methods—such old wine cannot be put into such new bottles. Time does not allow of my fully discussing this matter. I can only point out that the programmes of instruction on which the examinations are based are of such a nature as to make real instruction impossible—even if no other objection could be raised, the extent of ground to be gone over is so great as to make cram an absolute necessity. We have to bear in mind that Germany has prospered without such examinations, Japan also; I believe China is the only country in which a similar system meets with national support—recent events do not encourage us, however, to derive any consolation from this circumstance.

As Professor Herkomer points out: "Granted that the Kensington system was of use once upon a time, and that without it schools of art would not have been established at all, we must look the matter straight in the face and acknowledge that we have now arrived at a point when it must change its form in order to fulfil a great duty and to be of use, or else be disbanded". Undoubtedly this is so, and is as equally true of Science as of Art. The Department has at last perforce itself recognised the necessity of change, but all too slowly, and by appointing a certain number of inspectors has in a measure initiated a new policy. The very distinguished scientific man who is the Director of Science, in his evidence before the Royal Commission on Secondary Education, openly stated indeed that if he had his way he would entirely substitute inspection for examination in the elementary stage—but it is to be feared, unfortunately for the country, that in this, as in most other cases, one swallow does not make a summer.

Professor Herkomer insists that in the future freedom of action must be given to each master—to each town. This independence, he says, is to be obtained only by municipal and County Council aid. "Emancipation from the apron-strings of Kensington through municipal and County Council support would produce *an individuality*," we are told, "*in the art of each town*," for which I may substitute, *in*

the way in which science was taught and applied in each town.

But we must be careful that in leaving the science and art frying-pan we do not jump into a worse municipal fire, of which there is clearly some danger; for while all the world is engaged in decrying examinations, our County Council is bent on devising new ones. Scholarships at times are of great value to students, provided they fall into the right hands, and are obtained as well as held under right conditions. But it is easy to give too many scholarships, and still more so to give them to the wrong persons. Unless the examinations are placed in very competent hands, not only will a serious injury be done to our general system of education by leading those who are preparing boys and girls to adopt methods which it is unwise to follow in schools and to unduly force on their pupils, but the wrong people may be selected; the growth of a class of overtrained pot hunters may be encouraged instead of a vigorous, keen-witted, observant and resourceful race. Those who prove themselves the most apt scholars under the tutelage of the crammer, however able as desk workers, may in the end entirely disappoint the hopes of those who desire most to encourage the development of ability. Huxley has said much as to the importance to the nation of catching the potential Faraday, but it is doubtful whether such would ever shine in a competitive examination in which among other tasks they were asked to write an essay on Oliver Cromwell, or some other like topic equally remote from the daily experience of a healthy lad. If we depend too much on examinations we may easily select the unfittest for the work of the world, and unless very careful we are almost bound to select but one kind of ability—clerical rather than practical ability; unless indeed we altogether change our system of school education, and examine very differently.

It is also difficult to understand what is to be gained by examining candidates for £5 evening scholarships; it must prove to be a very expensive mode of distributing such doles, and it ought to be possible to find some other more prac-

tical way of selecting those whose studies would be materially promoted by such a grant.

Clearly, therefore, it is essential that we should not lose sight of the fact that an exceedingly complex educational system is now growing up under the influence of men who, for the most part, are in no sense experts, and have but little knowledge of the details of such work, although possessed with the desire in every way to do service to the community and to improve our national position. It therefore behoves all who can follow such work to keep most careful watch on the march of events; otherwise those who seek to benefit may in the end do irreparable injury; the present is a most critical period in our history, and such watchfulness is imperatively demanded of us.

I have ventured on this digression because so much depends on the foundation laid at school, as technical studies can only be satisfactorily engaged in by those who have been well trained from the beginning.

As Professor Herkomer says, the kind of individuality to be developed in each town—or in the case of our huge metropolis, in each district—will vary according to the necessities of the community. In future each Polytechnic in London must seek to ascertain what special work it can do to greatest advantage, instead of all following one example, as is too much the case at present. In words almost exactly those of my artist colleague: "This is the only way in which schools will obtain a direct influence over the industries of the country; and the influence will be the right one when the master is carefully selected, because it will be *the school around a man* and not a man struggling to be master in the midst of a system of impersonal teaching, where every student is expected to be squeezed into a great educational mangling machine". "Choose your master carefully," he says, "but then let him *be master*, and he will soon, with freedom of action, vary his forms of tuition according to the idiosyncrasy of each student, or the necessities of his immediate locality. The one true prize to be worked for would be individual progress. All teaching must be on a personal basis."

Choose your master carefully—this is indeed good advice. But this implies, of course, that those who have to choose know how—that they have some standard before them. Have they? Results seem to show that they rarely have. In this matter, as in many others I believe, the City and Guilds of London Institute has set a good example by selecting men known to be capable of doing research work, and a large amount of research work has been done in its Colleges. I am not aware that, excepting in the case of the Principal of this Polytechnic, capacity to undertake research work has been regarded as a qualification; on the contrary, for I know that when it was urged at one of them that a particular candidate had exceptional qualifications of this kind, the answer was: We want a man to teach, not to do research. But the work of true education *is pure research;* really good teachers are engaged in nothing else, being constantly occupied in studying their pupils' idiosyncrasies and in devising suitable methods of instruction. The "researcher" is the equivalent of the artist; the teacher who cannot engage in research is the equivalent of the inartistic copyist. No subject is at a standstill in these days—all progress involves research, although not always original research. The young child even is constantly engaged in research, and the habit is only gradually lost at school under our highly developed modern soul-killing system of perpetual lesson-learning, itself largely devised to satisfy a system of payment on results.

Let us hope, therefore, that every board of Governors will soon learn to appreciate the national importance of research, and will require evidence from every candidate for a teacher's post of ability in this respect—when such is the case, the research spirit will prevail also amongst students generally.

A most desirable example has been set in this direction by what, I suppose, may fairly be termed the least pretentious of the London Polytechnics—that in the Borough Road—which with the assistance of the Leathersellers' Company has just opened a branch tanning school at Harolds Institute, Bermondsey, and with unblushing

effrontery, one may say, prints on the programme under "Tanning School" the words *with special research laboratory*, and not content with this informs us on the next page that "the special research laboratory is fitted up and supported by the Worshipful Company of Leathersellers"—all hail to the Leathersellers, let us say! An industry which makes such a new start—very late though it be—and recognises the fact that research can help it out of its difficulties, is phenomenal in this country, but on the high road to retain its position if not to improve it.

I was much struck at the opening of the school by a statement made by the chairman, Mr. Lafone, M.P., who told us of an American customer who was in the habit of buying large quantities of a particular kind of leather here, of then taking it to America and manufacturing it, returning the goods here for sale. This man had remarked to him, he said, "that he had seen all our works and did not care a fig for our competition—for we had not even begun to know how to make the best". The introduction of the research spirit is sorely needed to cure such an old-world state of affairs as this.

Of course, whenever I advocate research in this way, and urge that the research spirit must be infused into all our teaching as well as into our national life, I am told it can't be done—that children can't solve problems. But there is a saying that an ounce of practice is worth a pound of theory—it is only a half truth, and a saying which is often misapplied, but it consoles me somewhat on such occasions. I *have done it* during the past fifteen years, since the opening of the Finsbury Technical College. No one has the right to say that it cannot be done until they have tried—all who really try, will succeed; those who do not, should not attempt to teach.

H. E. Armstrong.

RECENT WORK ON MOSSES AND FERNS.

THE Archegoniatæ (Mosses and Ferns) hold with regard to the progress of botany a position which might not inaptly be compared with that of the Low Countries in European history. The latter have been the fighting ground of nations: being in themselves objects of desire from their fertility, they commanded even a higher value as controlling the great waterways which lead into the very heart of Europe. And so the Archegoniatæ have been, and still are, the subject of much controversy; however interesting they may be in themselves, as organisms of beauty and of varied life-history, they appeal to the wider sense of the botanist as links of connection between the lower green Algæ, and the higher flowering plants. There is even a further attraction in them, from the fact that plants of this affinity are among the earliest of which we have any trustworthy knowledge derived from the palæontological study of the earth's crust, a branch of botanical science in which advances are rapidly being made. For these reasons, there is perhaps no part of the field of botany which commands more general attention than that which deals with the Mosses and Ferns. It happens that our eyes have been turned afresh to them by the recent publication of a work which, while it adds materially to the store of known facts, also brings forward fresh and weighty points for discussion (*Mosses and Ferns*, by D. H. Campbell: Macmillan & Co., 1895).

The first author who attributed to the Archegoniatæ their true position as connecting links, and showed on a broad footing of observation how their parts may be compared respectively with those of plants higher and lower in the scale, was Hofmeister; and it is not likely that any future writer will ever find it possible to compress into a single volume results which will have so wide and so permanent an influence as his upon this department of botanical thought. By him the comparative method was virtually

established, as applied to the details of development of the Archegoniatæ ; and subsequent work, even the best of it, has done little more than fill in the details of the design which he sketched in its broad outlines. But while Hofmeister, after the manner of a pre-Darwinian writer, proceeded on lines of pure comparison of form and structure, at the present time the tendency is towards a more physiological view. The attempt is being made to distinguish those characters which are most liable to change under varied external conditions from those which are more constant, and so to arrive at a more reasonable understanding of the true relations of organisms one to another as regards descent. What is readily mutable now under varied conditions has probably been mutable in the past also, and will be a less safe character for comparison than those features which persistently retain their form while the conditions vary. We shall see subsequently how important a bearing these matters have on opinion as to the relationships of the Archegoniatæ.

The point which emerged most prominently from the writings of Hofmeister was the phenomenon of "alternations of generations" in the Archegoniatæ. A feature of so wide a bearing as this may, with some certainty, be taken as the morphological expression of some far-reaching causes. However open to the accusation of guess-work, suggestions have been made as to the origin of this wonderful phenomenon, and the hypothetical story may be told in brief as follows :—

The origin of the great green-coloured series of plants, comprising virtually the whole of our land flora, was probably from aquatic forms resembling the green-coloured Algæ of the present day. Among the simplest of these we find that external conditions may directly determine the succession of events ; it has been shown in the case of *Vaucheria*[1] that the operator has it in his power to induce, by

[1] "Zur. Phys. d. Fortpfl. v. Vaucheria sessilis," G. Klebs. *Verh. d. Naturf. Ges. in Basel*, 1895. Also G. Klebs, "Ueber d. Vermehrung *v.* Hydrodictyon utriculatum". *Flora*, 1890.

exposure to suitable conditions, either sexuality or vegetative propagation; an unlimited number of vegetative or sexual generations may be produced in succession, and the operator may at will stop the succession by inducing under suitable conditions the alternative. Here then is no phenomenon of "alternation of generations" in the true (antithetic) sense, since the very essence of this consists in an obligatory succession of certain phases which are not to be modified at will by varied external conditions.

Such an obligatory succession is, however, found in a rudimentary form in certain other green Algæ, as for instance in *Sphæroplea*, *Œdogonium*, and *Coleochæte*. Here it appears invariably the case that the fertilised ovum (zygote) does not germinate directly so as to produce a single new individual, but there is a division of the zygote into four or more parts; each of these on separation produces by germination a new individual similar to the parent. The *constancy* of this event would point to its having some deep-seated importance; but among these plants the theory is in advance of the facts, and we are waiting for the actual observation of details; it would nevertheless seem probable that the explanation of the constancy is to be sought for in the condition of the nuclei after fertilisation. We know in other more highly organised plants that the nucleus of the zygote is the result of fusion of the male and female nuclei, and that on division it shows, as the result, twice the number of chromosomes which appears in the divisions preceding fertilisation. Such an increase could not be carried on indefinitely through a series of generations, but a return is necessary in some way to the original nuclear condition before fertilisation. In higher plants this has been found to take place by a process of numerical reduction, and it is believed that this process is intimately connected with those divisions which form a constant factor in the life-cycle of such Algæ as those above named.

It may be noted meanwhile, in the absence of direct observations on the Confervoideæ, that peculiar divisions, which may well result in a reduction, are seen on the germination of the zygote in certain Conjugateæ (see

Klebahn, "Studien über zygoten," *Pringsh. Jahrb.*, xxii., p. 415, etc.).

Whatever the details of the process of reduction may be, and we must wait for extended and direct observations to supply them, it would seem probable that the divisions of the zygote in the Confervoideæ are connected with the process of reduction, and perhaps owed their origin to the necessity for such a change. Their existence appears to have supplied the material upon which evolution has worked, so as to result in one of the most surprising phenomena in the whole organic world,—I mean the alternation of generations in the Archegoniatæ. For we learn by comparative study to believe it probable that the originally simple neutral generation became elaborated into the extensive body of the sporophyte; a stage was thus intercalated in the life-cycle, and becomes more prominent as the series of archegoniate plants is reviewed in ascending order; the progression culminates in the final triumph of the neutral generation as it is seen in the highest flowering plants.

The circumstances which led to the gradual advance from the few neutral cells which result from the division of the fertilised ovum as in the Confervoid Algæ to the larger neutral generation of the Bryophyta (sporogonium), then to the independent and still more complex sporophyte of the Pteridophyta, seem to have been as follows: Fertilisation is in all these organisms effected through the agency of water; since in aquatic organisms this may take place at any time, increase in number of individuals *may* take place without intermission; and this may, and in some cases apparently does, meet the needs for maintenance of the race. But if such organisms migrate to the land, escaping thereby it may be from inconvenient competition, or those conditions being in other ways more favourable, a check would at once be imposed upon unlimited sexuality; since water would only be present at intervals of rain, dew, or submersion, it would only be possible then to carry out the sexual process. As against the restriction of increase thus imposed, we may recognise the increased number of divisions succeed-

ing fertilisation; an increased number of new individuals being thus produced, while the dryness of the spores when ripe, which is a common character of the large majority of the Archegoniatæ, would facilitate their spread by winds and other agencies. In homosporous forms the spread and survival of the species is secured by production of numerous small spores, rather than by any special development or sexual differentiation of the spores themselves.

But the larger the number of spores produced, the greater will be the demand for nutrition by the parent. This demand we see in part met by the adaptation of the parent sexual plant to the conditions of land-life: the Marchantieæ show this in the elaboration of structure of the thallus which still maintains its simple outline, and it is most interesting to note how, by a perfectly distinct mode of development, a condition is attained in the highest of them (*Marchantia*, etc.) which is not unlike that of the leaves of some angiospermic plants. In other cases elaboration of outward form takes place, as in the Jungermanniaceæ and in the true Mosses (Musci); these sometimes simulating in a curious way the forms of the sporophyte in higher types.

But though the sexual plant might thus become elaborated to subserve more liberal nutrition, this could not suffice altogether, even in cases where a number of female organs had been fertilised on the same individual. An increase of number of spores produced from a single sporogonium necessitates increased facility of conveyance of their nutrition to them during development, and of their dispersion when mature; and in the undoubtedly ascending series of the Liverworts we see most beautifully shown the advance of complexity of the fruit body (sporogonium) to meet these needs; in Professor Campbell's book, pp. 21-151, readers will find the facts collected and arranged so as to illustrate the progression from simpler to more complex types of the sporophyte in these plants. There is good reason to believe, as the result of comparison, that there has been a process of *sterilisation* at work, converting cells which were potentially

sporogenous into cells with a mere vegetative function. For the outer protective wall the evidence of such origin is not conclusive, but for cells lying internally the chain of facts is striking. From the simple *Riccia*, where all the internal cells develop as spores, the most gradual steps of advance lead on to those forms which have a sterile foot, and elaters scattered through the mass of spores; and again, to those in which there is a considerable mass of sterile tissue forming a body like an incomplete columella (*Aneura*, *Metzgeria*, *Pellia*, etc.). The details of these are, however, more completely described by Goebel (*Flora*, p. 1, etc., 1895), and it appears from his drawings that the fertile tissue becomes localised in these forms as an irregular sporogenous band, surrounding the central but incomplete columella of sterile tissue; this is a very material advance in the direction of complexity.

A special chapter is devoted by Campbell to the Anthoceroteæ, which important order he is disposed to remove farther from the Hepaticæ than other writers, and he indicates for it a place "intermediate between them and Pteridophytes". With a simple type of gametophyte, these plants have a sporogonium which consists largely of sterile tissue; it is green, contains chlorophyll, has stomata, and appears thus as an efficient assimilating body. We thus see the neutral generation advanced towards a physiological independence, which is combined with a long-continued growth; for water supply, however, it is still dependent upon the gametophyte, having no root-system of its own. Internally the fertile tissue is more strictly localised than in any other Liverworts; it is referable to a definite layer surrounding the columella, which Campbell shows to be a constant feature. Two points are to be further noted: (1) the archesporial cells are ultimately referable in origin to superficial cells, this is the case also for all Pteridophyta; (2) the whole archesporial layer does not form spores, but sterile cells are intermixed with the fertile, so that the latter may appear in partially isolated groups (*Notothylas*), while the intervening sterile tissue reminds us of the trabeculæ of *Isoetes*.

From what has now been said it will be seen that as regards the sporophyte the Anthoceroteæ show an advance on other Liverworts and approach the Pteridophyta: they appear in fact the most probable link we possess between Bryophytes and Pteridophytes; though we recognise this, the gulf between them is still a very wide and deep one.

The Mosses (*Musci*) have been by various writers held to occupy an important position in the main line of descent of the Archegoniatæ, and it is only in comparatively recent times, and as the result of late work, that the opinion has gained ground that they are, as Campbell has expressed it, "a very clearly defined class, and that their relationship with other forms is at best a somewhat remote one" (*l. c.*, p. 216). "The Mosses, like the foliose Liverworts, seem to represent a modern, extremely specialised type, with no direct connection with higher forms. Undoubtedly related to the Anthoceroteæ through *Sphagnum*, their further development has diverged farther and farther away from the other Archegoniatæ, until in the Bryineæ both gametophyte and sporophyte have little in common with them." The grounds for this change of opinion must be carefully considered.

Since the time of Mettenius the very obvious comparison had been drawn between the protonema of Mosses and the filamentous prothallus of the Hymenophyllaceæ: the delicacy of texture of the leaves appeared to support this comparison. It was extended, on an enlarged basis of facts, by Goebel (*Ann. du Jard. Bot. de Buitenzorg*, vii.); and if, with him, we assume that the oldest archegoniate forms were filamentous, with the sexual organs directly inserted upon a branched cellular filament, it would appear that the Filmy Ferns, and especially *Trichomanes*, approach such a primitive form: at the same time the affinity of these to the Mosses would appear to be indicated, and in *Buxbaumia* Goebel believes that he has found the most primitive form of Moss. As long as we proceed mainly upon the mere formal comparison of the gametophyte, and relegate considerations of the structure of the sexual organs

and of the sporophyte to the background, and pass over the direct effect which external conditions have in modifying the form of the parts upon which our comparison mainly rests, this conclusion may appear satisfactory, and we may continue to regard the Mosses and Filmy Ferns as the nearest points of contact across the gulf which separates the Bryophyta from the Pteridophyta. But before acceptance of this position it will be necessary (1) to be sure that the characters upon which the comparison rests are reasonably *constant*, and (2) to be sure that the characters which we put only in the second place are really less important.

As early as 1888 the want of constancy of the vegetative characters of the prothallus in *Trichomanes* had been remarked upon (Bower, *Annals of Botany*, vol. i., p. 289, etc.). It was pointed out that "abnormal external conditions may largely control the vegetative development of the oophyte" (*l. c.*, p. 292), and that, therefore, in dealing with the decadent sexual generation, even greater caution would be necessary than in comparison of the nascent sporophyte. The inconstancy of form of the protonema in Mosses, and occurrence of flattened expansions in certain species, were a matter of old observation. The opinions based on these and other isolated observations have since been fully justified by the excellent work of Klebs (*Biologisches Centralblatt*, p. 640, 1893): this author has shown by cultures how direct is the influence of light upon the conformation of the gametophyte, and that the filamentous form can be extended or checked according to the intensity of the light to which the growing organism is exposed. We are thus driven to conclude that that which is so readily and directly mutable at the present day cannot be accepted as in itself sufficient evidence of a phylogenetic history which dates back, as the origin of Ferns certainly does, to a time prior to the Devonian period.

The comparisons based upon the filamentous character do not accord with those of the sexual organs; for instance, though the filamentous protonema of *Buxbaumia* may

resemble the filamentous prothallus of *Trichomanes* (Goebel, *Flora*, p. 92, etc., 1892), still the archegonium of the former appears to be a typical Moss archegonium, while that of the latter is as typically that of a Fern. In dealing with the classification of the higher plants the principle has been observed of giving priority to the reproductive organs, and using the comparison of the vegetative organs as secondary. There is no sufficient reason for departing from this method in the present case; if the attention be fixed on the archegonia, the distinction between Mosses and Filmy Ferns remains as wide as ever. Comparative study has taught that the archegonium is a relatively constant organ for the larger groups of plants, and its diagnostic importance should not be subordinated to the claims of the plastic vegetative gametophyte.[1] In addition to this there remains the vast difference between any Moss sporogonium and any Filmy Fern; and when this is added to the difference of archegonia and of

[1] It may here be noted that I have had fruiting specimens of *Buxbaumia* under observation during the summer of 1895. When they came to my hands in May, the sporogonia were of various ages: examination of a number of plants showed that, though the young sporogonia must have been making large demands for nutrition, no chlorophyll-containing tissue was present on those gametophytes which I examined; but there was as usual a very extensive system of rhizoids: there was chlorophyll in the older sporogonia.

Now the sporogonium is a large one, and the question is whence it obtains its nutritive supply. Its own powers of assimilation are not great, if we may judge from the extent of its chlorophyll-containing tissue, though this is larger than in most Mosses. But the question presents itself in its most acute form when the sporogonium is young, and this limited assimilating tissue not yet matured. In the absence of assimilating tissue of the gametophyte (as far as I have been able to observe on my specimens), the only source of supply which I can suggest is from saprophytic action of the rhizoids, a suggestion which Haberlandt has already made as the result of observation of their details.

It is true that after cultivation for some weeks under a bell glass, well exposed to light, there was a slight development of chlorophyll-containing cells on the gametophyte, but as these only appeared subsequently, at a time when the sporogonia were making no further demands for nutrition, they cannot be considered as having any direct bearing on the question. Though not conclusive, the evidence seems strongly in favour of saprophytic nutrition of *Buxbaumia*. F. O. B.

embryology, a very strong body of rebutting evidence appears to oppose the recognition of affinity of Mosses and Filmy Ferns on the basis of the plastic gametophyte.

The Ferns form a natural series (Bower, *Annals of Botany*, vol. iii., 1889). At the one end we find the eusporangiate types with massive meristems and sporangia, the latter being often associated to form synangia; the sexual organs are deeply sunk. At the other end of the series are the Leptosporangiate types, with more definite segmentation of the meristems, attenuated and separate sporangia, and projecting sexual organs. The peculiar parallelism of all these characters indicates that the series is probably a natural one. The question will be, which are the more primitive forms? The only *direct* evidence is from the rocks, and however insufficient this may be, it points clearly to an early preponderance, if not even an actual priority of the eusporangiate type. With this are to be coupled other considerations:—

(1) That as the Leptosporangiate Ferns are the only plants with sporangia originating from a single cell, and since the Lycopods and Equiseta are probably quite distinct stocks, the probability of some at least of the eusporangiate forms having been primitive is a strong one.

(2) That an increasing complexity of form was certainly a leading feature of the progression from any Bryophyte to any Pteridophyte, and the steps from a synangial sorus to a sorus with separate sporangia would have involved a similar increase of complexity of form; it would be a progression in harmony with the general spirit of the advance.

(3) That those who uphold the Leptosporangiates as primitive have not yet given any reasonable explanation of how that condition came into existence.

(4) That the hypothesis of septation resulting in synangia is in accordance with developmental data; it is analogous to what is actually seen in the septate anthers of Angiosperms, and it is clearly foreshadowed in the Anthoceroteæ. We are therefore able to give a reasonable suggestion how synangia may have arisen.

On these grounds (and other considerations might also be adduced) it would appear to be a reasonable view that the Eusporangiate Ferns were the more primitive, and that the Leptosporangiates are a later, specialised series, sharing, it is true, with the Mosses certain vegetative characters, though not on that account to be directly connected with them. Accordingly the nearest point of contact of Bryophyta and Pteridophyta is not to be sought for between Mosses and Filmy Ferns—both probably specialised types—but we should rather look to certain Hepaticæ, and compare them with Eusporangiate Pteridophytes. This suggestion came primarily from Campbell (*Bot. Gazette*, Jan., 1890), and the most prominent novelty in his book is the treatment of the whole Archegoniate series with this end in view. This has led to considerable changes in the usual arrangement of the materials, and, in accordance with opinions explained by Campbell in his text, he places the Ophioglossaceæ nearest of Pteridophytes to the Bryophyta. We do not propose here to enter upon a detailed criticism of Campbell's views on these or other questions; they wil doubtless be suitably reviewed elsewhere. We would rather consider the broad results which may be expected to follow from such a change of attitude as he has been the first to advocate, and his book the first to embody in the form of a general statement.

In the first place we see the sporophyte again reinstated in the position which it should hold as an important factor for comparison. The tendency in these borderland speculations has of recent years been to appreciate the decadent gametophyte, and depreciate the nascent sporophyte—as morphological currency. We would not advocate the converse; nor do we wish to see botanists ranged in two camps upholding respectively their favourite generation as the true basis of phylogenetic speculation. What we deprecate is the somewhat unsportsmanlike flight before the difficulties of comparison of the neutral generation. But the fact remains that in the long run it has become the dominant factor in plant-life, and its characters should therefore be given due weight.

Accordingly, while we keep the sexual generation, and especially the sexual organs themselves, constantly in our view, we must address ourselves seriously to reconsider the position as regards the sporophyte. Everybody at the present day is aware of the great differences between the Bryophyta and Pteridophyta. The most essential facts may be summed up in a single sentence; while all Bryophytes have a simple form of the sporogonium, and connected archesporium, Pteridophyta have differentiated vegetative members, and the sporogenous tissue segregated into separate cells or cell-groups. The distinction dates back at least to the period of the primary rocks; obvious intermediate links are absent. On grounds of comparison such as those advanced by Hofmeister, botanists believe that the sporogonium of the Bryophyte is truly homologous with the Pteridophyte plant. But we do not know whether the one was derived from the other; they may represent the results of two parallel but distinct lines of descent from some remote and unknown common ancestor. Thus stated the problem of origin of the more complex sporophyte seems hopeless indeed, for the earth's surface has been almost ransacked; in the last quarter of a century more country has been opened up for scientific inquiry than perhaps in any like period before, and yet no obvious connecting links have been found. As a foundation for opinions we must therefore return to the study of the organisms we know, and especially to those rare, isolated, and presumably antique types, which for various reasons we believe to be nearest to the border line. We must also take into our view such testimony as the rocks are able to provide.

Examining the simpler organisms on the Bryophytic side of the gulf, readers of Campbell's book will clearly apprehend the process which has been at work, how a progressive sterilisation of cells provides vegetative tissue of the sporogonium, so necessary for nutrition and dispersal of the enlarging mass of spores, or, to put the matter in a different terminology, certain cells in the course of descent lose the faculty of undergoing the reducing of chromosomes,

and remain as somatic cells. In the Liverworts as we pass to the higher terms of the series, we see as in the Anthoceroteæ the spore-mother-cells entangled in a mesh-work of sterile cells; here is truly an approach to a septate condition, but even here the final step is not taken.

With these ideas before us we pass to the organisms on the other side, and inquire whether in them there is evidence of sterile cells dispersed through the sporogenous mass: of incomplete septation: of formation of septa in organisms whose near relatives show none: or of actual conversion of tissues, of common origin and position with those which form spores, into sterile masses. Such evidence there certainly is, though much of it is open to various interpretations.

First, it is essential to obtain proof that complete septation can, and does occur. This is shown in the case of certain anthers of Angiosperms: in the Onagraceæ the anther is commonly of the quadrilocular type, but in certain genera, not differing otherwise in vital points, the loculi are found divided by sterile septa into two or more cavities: the tissue which forms the septa has been traced as of common origin with the sporogenous cells. The conclusion is therefore justified that in these cases *sterile partitions have been formed by conversion of potential sporogenous tissue into tissue of the septum.* Such a process is found also in the Mimoseæ, Myrsineæ, Rhizophoreæ, Loranthaceæ, Rafflesiaceæ: it is therefore no isolated phenomenon, but one of wide occurrence in Angiosperms.

The result is a partitioned body, more or less like the synangia which are so common a feature in certain Eusporangiate Pteridophytes, such as *Ophioglossum*, *Danæa*, *Psilotum* and *Tmesipteris*. It would lead us too far to go into the detailed evidence with regard to these plants: it is, or will be, fully set forth elsewhere. From study of various Pteridophyta we arrive at the position that sterile cells are frequently found dispersed through their sporogenous group (*Equisetum*, *Ophioglossum*, *Psilotum*, *Tmesipteris*, compare Liverworts); that incomplete septa, such as those present in certain of the anthers above quoted, are found also in

Pteridophytes (*Tmesipteris*, *Dancæa*, *Ophioglossum*); that potential sporogenous tissue is converted into sterile tissue (trabeculæ of *Isoetes*), and conversely tissue of a septum may become sporogenous (abnormal *Tmesipteris*). From these facts we conclude *that the limits between sterile and fertile tissue are not absolutely fixed, but that the two are mutually convertible;* further, *there is good reason to believe that a progressive septation has played a part in the production of the numerous sporangia of Pteridophyta:* how prominent a part it has played must still remain an open question.

The admission of the process of septation as a probable factor opens the way for fresh views of relationship. During the last quarter of a century various writers have noted the similarity in position of the fertile spike of *Ophioglossum*, and the sporangium of Lycopods: if the latter underwent septation, a body something like the former would be the result. The development has now been found to countenance such a view, while on a wide ground of comparison, morphological, histological, comparison of the gametophyte and embryology (as far as known), it appears that the view is supported. It would not be advisable, however, to carry such speculations further at present, and it must constantly be remembered that other factors exist. It is probable that fusion of sporangia may have occurred in certain cases as exemplified by the ovales of *Loranthus:* it is possible that there may have been in some cases a return to the faculty of spore-production in parts which had previously been vegetative. It must remain for future investigation to decide how far these various factors or others may have been active in producing the numerous sporangia of Pteridophytes as we see them.

But the second great point of difference between Bryophytes and Pteridophytes lies in the absence or presence of appendicular organs. No one can fail to recognise the enormous advantage which accrues from them; they first made physiological independence of the neutral generation possible by providing for successful assimilation, fixture in the soil, and more ready dispersal of the spores when

produced; while permanence by perennation was a general consequence of it.

The importance of this novelty may seem a ready explanation of the absence of the earlier steps of its progress; just as, in the perfecting of bicycles or electric lighting, the earlier improvements soon fall out of the competition, and disappear before their more advanced successors, so with most of the primitive leafy Pteridophytes. However little we may be able to pronounce upon the history of genesis of the primitive leaf (or its *modes* of origin, for they may not all have conformed to the same original type), there is one point which is prominent. There is a certain *correlation* between spore-production and vegetative development; where sporangia are borne, the vegetative development is restricted, but the latter is greater in their absence. Some will say, "This is a mere phenomenon of nutrition," and think that the matter may there be dismissed. But the development of all parts of plants is in a sense "a phenomenon of nutrition," and none the less calls for morphological study. The question will arise with regard to these correlative parts, is either the more primitive foliar form, and if so, which? It has commonly been assumed—in accordance with that uncertain guide, the theory of recapitulation—that the prior existent in the individual (foliage leaf) was the prior existent in the race, and that the sporophyll is therefore an altered foliage leaf. Others may, however, be disposed to hold a converse view, and assert that the foliage leaf is a sterilised sporophyll. There certainly would appear to be a strong element of *a priori* probability in the latter view, in the fact that spore-production was the first object of existence of the sporophyte; the production of spores is the end to which it always tends (leaving on one side rare cases of apospory); and the spore-stage is a constantly recurring event in the life-cycle. This we have every reason to believe has been the case throughout the course of evolution. As regards the history of the sporophyte, spore-production appears to have preceded any vegetative development of the sporophyte.

But in dealing with such a large and complex question,

applicable to so many and varied organic forms, it is not to be expected that all types could be covered by a simple statement; the separate lines of descent should be considered independently of one another, and so a general opinion be gradually formed. As matters stand at present, *it may be concluded with reasonable probability that in certain cases vegetative leaves have resulted from the sterile development of sporophylls.* But even if this were proved to have been their constant and only mode of origin (and we are far from being able to assert this) there would still remain the question of the primary origin of the foliar organs, as also of the root. On these points it must be frankly admitted that we are still in the dark, though their development and comparative study give some clues upon which hypothesis may rest.

We have thus briefly considered the two main points of difference between Bryophyta and Pteridophyta. Little has recently been gained as certain knowledge regarding the increased complexity of form, but the recognition of *septation*, as leading to a segregation of sporogenous masses, introduces an important factor. Though it does not in any sense solve the problem, this acceptance of an idea of septation, coupled with the view that the Eusporangiate Ferns were the more primitive, does appear to be a step towards its solution.

One direct result of Campbell's view of the Eusporangiate Ferns is, so to speak, to pollard the evolutionary tree. In the past, attempts have been made to deduce the various Pteridophyte types from Leptosporangiate Ferns, arranging them in extended sequence; this involved reduction on a large scale, "elimination" of the identity of the sporangium by fusion; the introduction of the idea of "sporocysts," a term applied first to those deeply-sunk sporangia of *Ophioglossum* which were believed to represent a fern sorus, not a sporangium (Strasburger, *Bot. Zeit.*, 1873). Such ideas of reference of the divers stocks to a single type have long been either loosely held, or rejected; but so long as the Leptosporangiates are considered the most primitive, there will always be a disposition to look, in other lines of

descent, for similar simple, and presumably primitive sporangial types. With the acceptance of the Eusporangiate Ferns as the more primitive, this tendency disappears; the large majority of Vascular plants are Eusporangiate; there is therefore no reason to attempt to refer them to any one Leptosporangiate ancestral form. We are prepared on reasonable grounds to recognise a veritable brush of lines of descent. Thus there is no need to look upon *Isoetes* as necessarily a Fern *or* a Lycopod; why should it not be a relic of an independent series not immediately to be ranked with either? The Ophioglossaceæ are not necessarily Ferns; is it not possible that they also may be the result of an independent evolutionary line?

A weak point in much of the discussion of Pteridophyta in the last quarter of a century has been the free resort to hypotheses of "reduction," or "fusion" of parts. The climax of this mode of explanation was perhaps arrived at in the paper above quoted (*Bot. Zeit.*, 1873), in which it is suggested that in the Ophioglossaceæ there has been "a contraction of the sori with reduction of the sporangia," in *Lycopodium* and *Selaginella* a "contraction of the whole fertile leaf segment of the Ophioglossaceæ to one sporocyst," and in *Psilotum* a "contraction of the whole *Lycopodium* strobilus to three sporocysts with reduction of the sterile leaf segment". I quote these as examples of the lengths to which hypotheses of reduction may go, not assuming that their author would uphold them in the present position of the science. In the game of whist the general maxim is sometimes formulated, "When in doubt play trumps;" and so in morphology one may almost recognise as the practice, "When in difficulties suggest reduction". But the strong whist player will only use his trumps with a definite object, and the morphologist should only suggest reduction when there is some reasonable physiological backing for such a suggestion. Hitherto this principle has not been sufficiently observed.

In the Leptosporangiate forms the spore-production is not complicated by the more direct influence of sexuality, nor by any marked need for storage of nutrition. The

chief object appears to be the production of as *many* matured spores as possible. Other things being equal there would thus be an *a priori* improbability confronting any suggestion of reduction; and on this account we should deprecate any free resort to this as an explanation of morphological difficulties in homosporous forms. It is more reasonable to suggest that it has occurred in heterosporous forms, for in these the tendency is not to mere numerical spore-production, but towards the perfection of a smaller number suitably equipped with nutritious stores, so as to tide over the organism more securely to the time when the embryo can manage for itself. We need not necessarily assert that reduction has never taken place in homosporous Pteridophytes; other circumstances may have conduced to it. But the dominant feature has no doubt been the production of as many spores as possible, and considering this circumstance we should look critically at any suggestion of reduction in homosporous plants, and only accept it as probable when some good physiological reason can be assigned for its having occurred.

From the above pages it will be seen that opinion on the Archegoniatæ is now in a state of flux. Little is definitely known, and the facts are often susceptible of various interpretation. To those who lay greater stress on one character a certain conclusion may seem clear; by those who select another character as more important, the converse conclusion may be arrived at, and be held with equal tenacity. The whole would seem to turn upon the selection of the characters to which weight is to be attached. Whatever may be the shortcomings of recent work, and recent argument, one point seems to emerge with clearness. *Relationships, especially among ancient forms, are not to be based upon comparison of characters which are directly variable in a high degree in the individual under changing conditions of present life.* It has been conclusively shown that the vegetative characters of the gametophyte are often so influenced in Mosses and Ferns, and we accordingly conclude with some degree of certainty that relationships based on that foundation are insecure, and especially so

when they do not acquire substantial support from comparison of less variable parts, such as the archspouia. We shall be disposed therefore to agree with Professor Campbell in his main point. But it must not be thought that the priority of Eusporangiate forms is *proved;* few cases are proved or susceptible of proof in morphological discussion. The most that can be done is to show a preponderance of probability, and this we believe has been done in the present case.

F. O. Bower.

THE NEW THEORY OF SOLUTIONS.

(*Continued from vol. ii., p.* 184.)

CONCENTRATED SOLUTIONS.

MOLECULAR weights and concentration. — The extensive measurements which have been made of the molecular weights of dissolved substances are at one in showing that, no matter what method is used or on what property the method depends, the values obtained for the molecular weight of the same substance when dissolved in the same solvent vary with the concentration of the solutions employed. The extent of this variation depends both on the nature of the solvent and the dissolved substance, but for the great majority of indifferent solutions the order of magnitude of the variation is somewhat the same, and is illustrated by the numbers given in the following table. The data, determined by Beckmann, refer to the freezing-points of solutions of alcohol in acetic acid, and show that, as the concentration varies from ·2 gr. to 14·2 gr. of alcohol per 100 gr. of solvent, the molecular weight, calculated by the ordinary freezing-point formula, varies between 47 and 58, the theoretical value being 46.

g	ΔT	M′
·254	·210	47
2·808	2·110	52
6·194	4·490	54
9·687	6·770	56
14·20	9·550	58

Instead of expressing the concentration, as above, in grams per a definite weight of solvent, it may be expressed, as in the case of gases, in grams per a definite volume of solution. With dilute solutions these two methods give practically the same results, and the former, due to Raoult, is almost exclusively used in laboratory methods of determining molecular weights, since the latter, first used

by Arrhenius, involves, in addition to the other data, a knowledge of the density of the solution. In the case of concentrated solutions, however, the two methods may give results which differ considerably, as is shown in the following table. Here are given in the last column the values of the molecular weight of alcohol obtained from the above data on using in the freezing-point formula, instead of g, values of g' or the number of grs. of alcohol in 100 cc. of solution.

P	g'	M″
1·3	·266	47
14	2·853	50·2
30	6·046	50·0
45	9·079	49·8
62	12·65	49·1

The values of M″ now vary by only some 3 units. In general, however, the method of Raoult gives values which vary less with the concentration than those given by the method of Arrhenius. This is well illustrated by the recent work of Abegg (1894) on the freezing-point of very strong solutions. Neither method is theoretically perfect, Raoult's being probably the more correct. It may be taken, however, that the above is a fairly typical instance of the extent to which the molecular weight varies with the concentration.

It is of interest to compare this variation with that shown by the molecular weights of gases at different concentrations. If we calculate the osmotic pressure of alcohol in the above solutions by the equation which holds good for a dilute solution, the values in atmospheres thus obtained are given in the first column of the preceding table. Even in the weakest solution the osmotic pressure is greater than one atmosphere, and in the strongest it is greater than sixty atmospheres. Now it is well known that if, by the equation which applies to a gas of small concentration, we calculate the molecular weight at pressures differing as widely as the osmotic pressures of the above solutions, here also there is considerable variation in the values obtained. This is

shown in the following table based upon observations made by Roth (1880) on ethylene and carbon dioxide. P is the pressure of the gas in atmospheres, g' its concentration in grs. per 100 cc., and M″ the molecular weight.

P	Ethylene.		Carbon Dioxide.	
	g'	M″	g'	M″
1	·1175	28	·1922	44
9·90			2·078	47·6
14·79	1·859	29·5		
29·65	4·137	31·4	7·267	55·4
39·49	5·949	35·4	10·80	61·8
49·34	8·160	38·9	12·05	55·2

The values of M″ are calculated on the assumption that the gas obeys the ideal laws, and, according to the kinetic theory, these laws apply only to gases in which the attractive forces in play between the molecules are inappreciable, and for which the actual volume of the molecules may be neglected in comparison with the volume of the gas. To these disturbing influences, which must of course become more and more important as the pressure increases, are ascribed the variation in the values of M″, and van der Waals' corrected gas equation, which takes note of these influences, approximately expresses the behaviour of compressed gases.

Theory of concentrated solutions.—Although the theory of concentrated solutions cannot yet be adequately tested experimentally, it is of service in explaining some of the foregoing facts. By reasoning similar to that used by van der Waals for gases, it follows that, owing to the attraction of dissolved molecules for each other, the osmotic pressure of a dissolved substance will be less than what it would otherwise be by an amount proportional to the square of the concentration. Acting in the opposite sense to this attraction will be the attraction between dissolved and solvent molecules. The latter attraction will increase the osmotic pressure by an amount which according to Arrhenius (1892) is also proportional to the square of the concentration. This

somewhat surprising result is opposed to the assumptions made by Bredig (1890) and by Noyes (1890) in their theoretical treatment of strong solutions, and is arrived at in the following way. Consider a molecule of dissolved substance in a plane surface separating solvent from solution—such a molecule as would be exerting osmotic pressure on a semipermeable membrane separating the two. Of the molecules attracting the molecule under consideration the solvent molecules in the solution and those in the pure solvent will pull it in opposite directions. Symmetrically situated with respect to the plane there will be a molecule in the pure solvent corresponding with every solvent molecule in the solution, and the attractive effects of these two sets of solvent molecules on the molecule in question will balance each other. Consequently, the only solvent molecules which affect the molecule in question are those in the pure solvent which, as regards the plane, are placed symmetrically to those of the dissolved substance. The number of operative solvent molecules is proportional, therefore, to the concentration of the *dissolved substance*, and, since the total number of dissolved molecules in the plane at any instant is also proportional to the concentration, the total attractive effect between solvent and dissolved molecules is proportional to the square of the concentration. It follows, therefore, that setting aside polymerisation of dissolved molecules, the osmotic pressure P of a solution of volume V at the absolute temperature T is given by an equation of the same form as that of van der Waals—

$$\left(P + \frac{a}{V^2}\right)\left(V - \beta\right) = R'T.$$

Here, however, a, the attraction-constant, is a measure of the difference between the attractions of the dissolved molecules for one another and the attraction of solvent molecules for dissolved molecules; and β is a volume-correction depending on the actual volume occupied by the molecules of the solvent and the dissolved substance, since both of these will diminish the space in which the dissolved molecules can oscillate.

Now, in the case of even a weak solution it is evident that this volume-correction must be considerable. In the case of many liquids it appears that the actual volume occupied by their molecules under ordinary conditions is about one-third of the total volume, consequently, on using such a liquid as a solvent, the space in which the dissolved molecules may oscillate is much smaller than the volume of the solution. Why, then, should solutions conform so closely to the ideal gaseous laws? Why should values of the molecular weight even in strong solutions approximate to the theoretical value? Nernst has pointed out the reason by calling attention to the fact that the properties of solutions have not been investigated to any extent by direct measurements of osmotic pressure, but by indirect methods making use of freezing-point, vapour-pressure, etc. Now these methods are concerned with the separation of solvent from dissolved substance (see vol. ii., p. 184), and although correlated with osmotic pressure in the manner already indicated, they do not in reality give measures of the osmotic pressure but of the osmotic work, or the work required to remove unit volume of solvent from the solution. Let P be the gaseous pressure of a substance, and when the substance is dissolved in a solvent, other things remaining the same, let P be raised in the ratio of $P : P/(1 - \beta)$, where β is the volume-correction due to the molecules of the solvent. Even although this is so the osmotic work will still be P, for the change of volume which the pressure $P/(1 - \beta)$ has to effect is no longer unity but $1 - \beta$. For this reason the volume-correction does not affect the results of freezing-point, vapour-pressure, and other methods based on the same principle. These results are only affected by the attractive forces included in the a-term of the equation, and since the osmotic pressure will be larger or smaller than the theoretical value according as a is positive or negative, it follows that the molecular weight will increase or diminish with the concentration according as the attraction of the molecules of the dissolved substance for each other is greater or less than the attraction of the molecules of the dissolved substance for those of the solvent.

Such is the theory of concentrated solutions which evidently goes some way to explain the facts illustrated by the preceding examples. These show that, in general, the properties of strong solutions deviate from those of ideal solutions just as the properties of compressed gases deviate from those of gases at small concentrations; and further, that the deviations are if anything less marked in the former case, when the comparison is made under conditions which according to the gaseous analogy are similar.

ASSOCIATED SOLUTIONS.

Molecular weights in associated solutions.—To this general rule there exists a well-marked series of exceptions, which are comprised under the heading of associated solutions. The peculiarities of these solutions have mainly been brought to light by determinations of the freezing-point and boiling-point, and were discovered by Raoult. The large number of measurements of the molecular lowering of the freezing-point, published by him in 1884, proved that in solvents such as water, formic acid, and acetic acid, all indifferent substances gave what he termed "normal" values; but they also showed that certain compounds, like the fatty alcohols, the fatty acids, etc., when dissolved in solvents like benzene, nitrobenzene, or ethylene dibromide, gave lowerings which were abnormally small. Here are some of the molecular lowerings he obtained calculated from the theoretical molecular weights by the formula $M'\Delta T/g$.

Solutions in Benzene.			
Dissolved Substance.	$M'\Delta T/g$	Dissolved Substance.	$M'\Delta T/g$
Methyl Alcohol - -	25·3	Formic Acid - - -	23·2
Ethyl ,, - -	28·3	Acetic ,, - - -	25·3
Butyl ,, - -	43·2	Valeric ,, - - -	27·1
Amyl ,, - -	39·7	Benzoic ,, - - -	25·4
Phenol - - - -	32·4		

The "normal" value for benzene was 49, hence Raoult came to the conclusion that hydroxy-compounds when dis-

solved in certain solvents, particularly hydrocarbons, gave almost half the "normal" value. Now van't Hoff's theory and subsequent experiments have proved that the molecular lowering of a solvent has only one definite value which can be calculated from its freezing-point and heat of fusion in the manner already shown, and Raoult's so-called "normal" lowerings agree in all cases with the values calculated in this way. The abnormally small molecular lowerings are ascribed therefore to the exceptional behaviour of the dissolved substance, and find a probable explanation in the assumption that in these solutions its molecular weight is greater than the theoretical value. From the above results it would appear that the dissolved substance existed as double molecules; subsequent experiments by Beckmann (1888, 1890), Eykmann (1889), Paternò (1889), Auwers (1893-94), and others, have proved, however, that this view is misleading, because the molecular weight varies as a rule at an abnormally rapid rate as the concentration alters. Here are the results obtained by Beckmann with solutions of alcohol in benzene, the values of the molecular weight being calculated both by the methods of Raoult (M′) and Arrhenius (M″).

g	ΔT	M′	g'	M″
·164	·175	45·9	·144	45·8
1·088	·875	60·9	·946	60·2
3·483	1·705	100·	2·952	96·4
8·843	2·725	159·	7·096	145·
14·63	3·445	208·	11·10	179·

In the most dilute solution the theoretical molecular weight is obtained; in the strongest, however, the value is four times the theoretical number. The result arrived at by Raoult regarding the existence of double molecules is therefore due to the fact that he used one solution only, and did not study the effect of concentration.

On contrasting the variation in the values for M′ and M″ in the above table with that of indifferent solutions in general, as typified by the solutions of alcohol in acetic

acid previously considered, it is evident that some additional disturbing factor must operate in the former case, and the data clearly point to the existence in strong solutions of complex molecules of dissolved substance which become dissociated into simple molecules as the dilution increases.

Nature of the dissolved substance.—Now it is one of the most interesting facts discovered in this field that almost all associated solutions are solutions of hydroxy-compounds, and further, that the great majority of hydroxy-compounds when dissolved in hydrocarbon solvents give rise to associated solutions. The following tables, based on data summarised by Auwers (1893) illustrate the behaviour of some of the more important members of four different classes of these compounds, the solvent being benzene. Under g, is given the limits of concentration between which the observations were made; under M′, the observed limiting values of the molecular weight; and under $\overline{M}$, the theoretical molecular weight.

These tables show that in very dilute solutions the alcohols give normal molecular weights which increase at an extremely rapid and approximately constant rate as the concentration increases. The curves obtained on plotting molecular weight as ordinates against concentration as abscissæ are therefore almost straight lines steeply inclined to the horizontal axis. The shape of the curves for the acids indicates that, in general, in extremely dilute solutions the molecular weights would also be normal; the values increase very rapidly, however, as the concentration increases, until they are almost twice as large as theory demands; further increase in concentration is then attended by a slow increase in this double molecular weight of the same order as that usually occurring in the case of unassociated solutions. The curves for the oximes resemble those for the acids with the difference that the increase to the double molecular weight proceeds at a very much slower rate. The two lowest phenols give curves resembling those of the alcohols; the behaviour of the others is normal, although, with the exception of thymol, the effect of concentration cannot be accurately ascertained on account of their limited

solubility in benzene. To summarise, the curves for the alcohols and lowest phenols closely resemble those of complex molecules dissociating into simple molecules, while the curves for the acids and oximes resemble those of double molecules dissociating into simple molecules.

	g	M′	$\bar{M}$
Alcohols.			
Methyl	·32— 7·29	43—144	32
Ethyl	·16—32·45	46—318	46
Isopropyl	·97— 7·39	64—158	60
Octyl	1·52—15·26	138—312	130
Cetyl	1·31— 4·05	229—434	242
Benzyl	·92—20·70	118—271	108
Oximes.			
Acetophenone	·62—28·87	169—279	135
Benzophenone	·23— 1·56	211—260	197
Dextrocamphor	·54—25·50	238—313	167
Laevocamphor	·51—11·77	229—308	167
Dextromenthone	·37— 8·75	228—309	169
Laevomenthone	·58— 7·16	251—292	169
Acids.			
Acetic	·47—22·80	110—153	60
Propionic	1·36—17·68	131—140	74
Caproic	1·05—16·29	194—221	116
Caprylic	1·99—11·39	247—255	144
Lauric	·72—16·30	320—377	200
Benzoic	·27— 4·73	219—236	122
Salicylic	·21— ·40	182—209	138
Phenols.			
Phenol	·34—26·77	144—252	94
p-Kresol	1·15—18·38	107—208	108
Thymol	1·28—17·71	146—184	150
α-Naphthol	·26— 4·99	127—147	144
β-Naphthol	·13— 2·32	125—154	144

Independent evidence that in associated solutions we are really concerned with dissolved substances having abnormally high molecular weights is not difficult to find.

A study of the general physical behaviour of pure hydroxy-liquids, particularly such properties as surface tension, viscosity, vapour-pressure, boiling-point, and heat of vaporisation (see "SCIENCE PROGRESS," vol. iii., pp. 226-230), points to the conclusion that their molecules, as distinguished from those of liquids in general, are really aggregates of gaseous molecules. Hence it is not surprising that in solution there is evidence of these molecular aggregates, and that curves are obtained for their molecular weights at different concentrations resembling those of dissociating gases. Indeed, so definite seems to be the rule that such is the constitution of a hydroxy-liquid, and so uniformly does it give rise to anomalies in certain solvents, that attempts have been made to use observations on the freezing-point of solutions to ascertain in the case of tautomeric compounds of doubtful chemical structure whether or not a hydroxyl-group occurs in the molecule (Auwers, 1894). Why hydroxy-compounds behave in this way is difficult to say. The only hypothesis yet put forward is due to Brühl (1895), who concludes that in the simple molecules of these substances oxygen is tetravalent and that when they exist as simple molecules they are in reality unsaturated compounds. The only other substances which resemble them in giving associated solutions are compounds whose molecules contain a labile hydrogen atom.

Effect of temperature.—Very little work has yet been done to ascertain how the molecular weight of a substance in an associated solution alters with the temperature. The following are Beckmann's values of the molecular weight of benzoic acid determined from the freezing-point and boiling-point of solutions in benzene. The theoretical value is 122.

Freezing-Point.		Boiling-Point.	
g	M′	*g*	M′
·567	223		
1·444	228	1·49	231
2·602	232	3·28	244
4·725	236	5·15	255

In all except the most dilute solutions, the curves obtained on plotting the above values show that the molecular weights determined at the boiling-point are slightly the larger. Here no definite temperature effect is noticeable. No observations at the boiling-point have been made on solutions of the great majority of the other hydroxy-compounds, as they are too volatile to give satisfactory results using the ordinary experimental method.

Nature of the solvent.—Besides the nature of the dissolved substance the second main factor instrumental in producing an associated solution is the nature of the solvent. In general the properties of indifferent solutions give little or no indication of a specific action of the solvent on the molecular condition of the dissolved substance; the solvent apparently merely supplies a medium in which the molecules of the dissolved substance may distribute themselves. The properties of associated solutions clearly prove, however, that in certain cases the solvent cannot be regarded as passive. They show that whereas certain solvents, like water, acetic acid, etc., can resolve the complex molecules of hydroxy-compounds into simple molecules, other solvents cannot effect this decomposition. According to the work of Raoult, Beckmann, Eykmann, Paternò and Montemartini (1894), and Beckmann, Fuchs, and Gernhardt (1895), the following solvents may give associated solutions: benzene, nitrobenzene, paraxylene, naphthalene, diphenyl, diphenyl-methane, methyl-propyl ketone, alkyl bromides and iodides, ethylidene dichloride, ethylene dichloride and dibromide, chloroform, and carbon bisulphide; a less marked tendency to form such solutions is also shown by azobenzene, anethol, and benzophenone. These solvents are mainly hydrocarbons or their simple substitution products which are not of the water-type; other solvents, including, it has to be noticed, hydroxy-compounds themselves, like water, acids, alcohols, phenol, etc., give normal values for all indifferent substances if the solutions be dilute, although in concentrated solutions, even in water, indication of molecular aggregation can be detected (Abegg, 1895).

Little has up till now been done in explaining the dif-

ference in the behaviour of different solvents. Nernst has recently (1894) put forward the view that differences in the values of the dielectric constants of the solvents are an important factor. This idea is based on the fact that the electrical energy of a system of charged conductors is diminished on transferring it from one medium to another having a higher dielectric constant. For this reason, as J. J. Thomson first pointed out in 1893, if a molecule be regarded as composed of charged particles the forces in play between them will be diminished in a medium having a high dielectric constant, and the more readily, therefore, will dissociation occur. Although, as will be evident later, this reasoning can be directly applied to electrolytic solutions, it is only by analogy that it can be used when dealing with the action of a solvent on a molecular aggregate. Nevertheless, where the necessary facts are known, they show that there is a close parallelism between the dissociative action of solvents and their dielectric constants. The following table gives in order of magnitude the values of the dielectric constants at 15°, determined by Landolt and Jahn (1892), and by Thwing (1894), for the liquids commonly used as solvents. The positions of nitrobenzene and acetie acid are somewhat exceptional, yet it is plain that, in general, the solvents in which no association can be detected belong to one end of the series, while hydrocarbon solvents showing definite association fall at the other.

Solvent.	Dielectric Constant.
Water	75·5
Formic acid	62·0
Methyl alcohol	34·0
Nitrobenzene	32·2
Ethyl alcohol	25·0
Ethylene dichloride	11·3
Acetic acid	10·3
Chloroform	3·9
Carbon bisulphide	2·5
Paraxylene	2·4
Benzene	2·2

According to Brühl the reason why oxy-solvents do not as a rule yield associated solutions is due to their unsaturated nature; oxygen is here regarded as playing the part of a tetravalent atom, the "free valencies" of which being the real cause of the dissociative action of the solvents on molecular aggregates.

Comparison of abnormal molecular weights given by different methods.—It follows from the preceding discussion that, assuming Avogadro's hypothesis applies to dilute solutions, in certain solvents the molecular weights of certain dissolved substances are not identical with their gaseous molecular weights. The alcohols which have normal gaseous molecular weights give abnormal solution molecular weights. Both the gaseous and solution molecular weights of the acids are abnormal, but the two are not identical under conditions which, according to the gaseous analogy, are similar. Thus Nernst calculates that in solution in benzene, at 80°, acetic acid is dissociated to a greater extent than when it exists as gas at the same temperature and concentration; the reason he gives being that the dielectric constant of benzene (2·2) is greater than that of free space (1). This reasoning would not apply, however, to the molecular weights of the alcohols in solution and in the state of gas. Indeed, if we deal with substances which give abnormal molecular weights when gasified, in no case are these identical with the solution molecular weights, and the latter vary with the solvent. For example, chloral hydrate and alcoholate are partly dissociated on vaporisation, but when dissolved in benzene they give practically normal molecular weights in dilute solutions. In acetic acid they are partly dissociated into their proximate constituents in dilute solutions, and in water chloral hydrate gives normal values while the alcoholate is almost completely dissociated at all concentrations up to 18 per cent. The conclusion to be drawn from the foregoing facts is, therefore, that if the dissolved substance has an abnormal gaseous molecular weight, or if in the liquid state it consists of molecular aggregates, the solution molecular weight differs from the gaseous molecular

weight. These two values agree, however, if the substance when gaseous and when liquid consists of simple molecules.

It is also of interest to compare the molecular weights obtained from associated solutions with those determined by independent methods for the liquid molecular weights of the pure dissolved substances. The only property which has to any extent been used to give an idea of the degree of molecular aggregation is surface energy. From measurements of this property Ramsay and Shields (1894) conclude that at 20° the average liquid molecular weights of methyl alcohol, ethyl alcohol, and acetic acid are respectively $(CH_3OH)_{2\cdot32} = 74$, $(C_2H_5OH)_{1\cdot65} = 76$, and $(CH_3COOH)_{2\cdot13} = 128$. On referring to the table on p. 385, it will be seen that although in benzene the solution molecular weights of acetic acid are not far removed from the above value, even in moderately dilute solutions the molecular weights found for the alcohols are much larger than those given for the pure liquids by surface-energy observations. A more extended comparison of the two sets of numbers shows that, in general, the results are not in harmony; as a rule the solution molecular weights are the larger, and hence if the liquid molecular weights deduced by Ramsay and Shields are correct, in most cases the solvent cannot be regarded as merely breaking up the complex molecules of the pure liquids into simpler molecules, but as promoting further aggregation. In a few cases the opposite effect is exerted. Thus, according to surface-energy observations, acetone, nitroethane, and propionitrile are associated liquids, but in benzene they give normal molecular weights. Of course the agreement between the results of the two methods could not be otherwise than imperfect in the present state of the questions. Because, on the one hand, the values of the molecular weight given by strong solutions are not the result of molecular aggregation alone, but are affected by the attractive forces already considered; and, on the other hand, the molecular weights derived from surface-energy observations can hardly be regarded as anything but approximations, since the method of calculation is not free

from objection, and does not rest on a firm theoretical basis (see "SCIENCE PROGRESS," iii., 229). What appears conclusively settled is, that dissolved substances producing associated solutions when pure and in the liquid state consist of complex molecules; and one interesting point, at present in an unsatisfactory state, is whether the aggregation of the liquid when pure is not always greater than when it is dissolved.

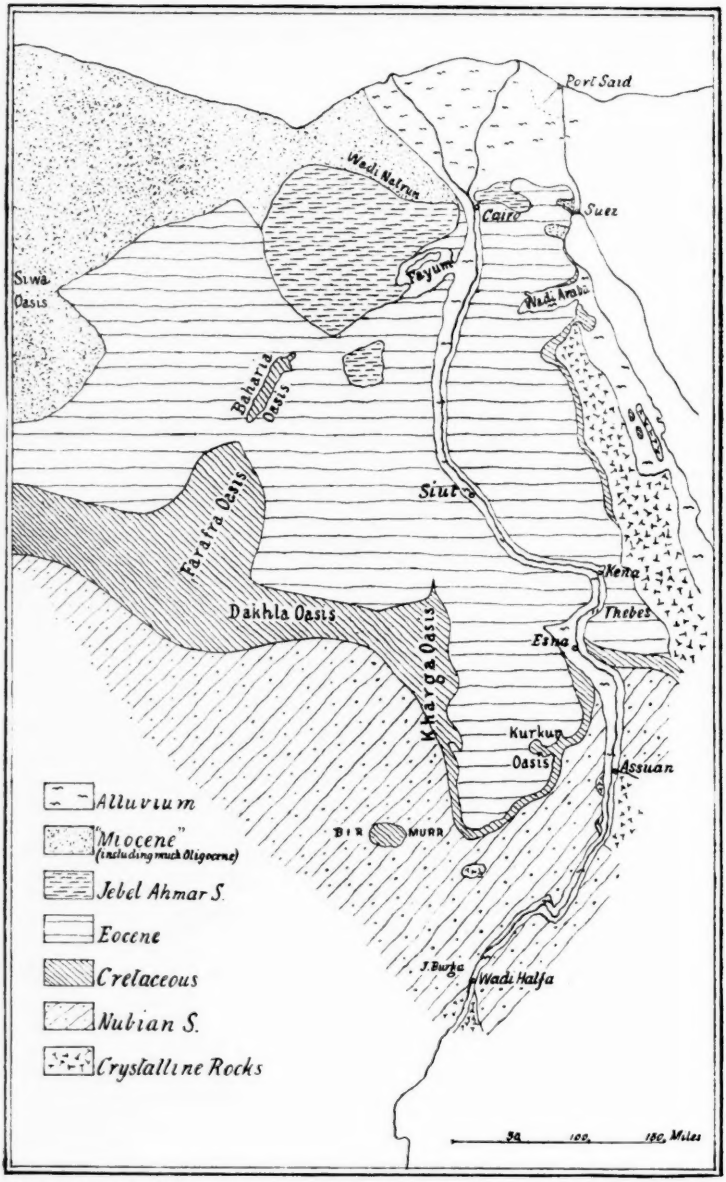
Anomalous freezing-points.—It sometimes happens that, as regards their freezing-points, certain solutions simulate the behaviour of associated solutions. This is due to the fact, first pointed out by van't Hoff, that, on freezing the solution, dissolved substance solidifies along with the solvent, and of course in this case the ordinary freezing-point laws are no longer valid. In this way have been explained the low values of the molecular lowering, and high values of the molecular weight found for m-kresol in phenol, and aldoxime in acetoxime, observed by Eykmann (1889), and thiophen in benzene observed by Paternò (1889). It has further been shown that the tendency to form mixed crystals on freezing is most marked in the case of isomorphous substances, and Kuster's work (1890-91) has proved that the freezing-point of an isomorphous mixture may in some cases be calculated from those of its constituents by the ordinary admixture rule. In such a case, if the freezing-point of the dissolved substance is higher than that of the solvent, its presence will bring about a rise instead of a fall in the freezing-point of the solvent. This behaviour was observed by Heycock and Neville (1889) in the case of a solution of antimony in tin, and by Eykmann for β-naphthol in naphthylamine. Van Bijlert (1891) in these and other cases proved by actual analysis that the ice separating from the solutions really contained dissolved substances.

An interesting development of this question is due to Ferratini and Garelli (1893), who discovered several anomalous solutions of this class, and pointed out that in all probability similarity in the chemical constitutions of solvent and dissolved substance, rather than similarity in their

chemical behaviour, led to the formation of mixed crystals. For example, the molecular weight of quinoline is normal in benzene, but abnormal in naphthalene; acridine gives a normal value in benzene, but an abnormal value in phenanthrene. Such observations may be used to throw light on the chemical constitution of dissolved substances. Nicotine, for instance, unlike γ-dipyridyl, does not form mixed crystals with diphenyl, consequently the constitution of nicotine, which is at present undecided, is probably of a different type from that of γ-dipyridyl (Garelli, Ciamician, 1894).

J. W. RODGER.

(*To be continued.*)



GEOGRAPHICAL SKETCH-MAP OF EGYPT. (Principally after Zittel and Lyons.)

THE GEOLOGY OF EGYPT.

IN the account of the geology of the Sahara which appeared in "SCIENCE PROGRESS" for July, 1895, the description of the Egyptian deposits was necessarily very cursory. But Egypt is geologically a part of the Sahara, and no account of the latter can be complete without including the former. The present article, therefore, is practically a continuation of the previous one.

The literature of Egyptian geology is so extensive that it would be impossible, even if it were desirable, to attempt here a complete bibliography of the subject. I have, therefore, quoted only those works to which it is necessary to refer in order to give a fairly complete description of the geology of this region. Even yet by far the most detailed general account of the country is to be found in Zittel's *Libysche Wüste* (34). But this was published in 1883, and since that time much geological work has been done in Egypt, especially in Lower Egypt and in the Arabian Desert. References to the most important papers which have appeared since the publication of Zittel's work will be found in the bibliography at the end of this article.

Egypt, in the ordinary sense of the term, includes the region which lies between the sandy part of the Libyan Desert on the west and the Red Sea on the east, and which extends from the shores of the Mediterranean to the "Second Cataract" near Wadi Halfa. It is divided into two parts by the Nile; and excepting the delta and the valley of that river, it is for the most part a barren waste of mountain and plateau. West of the Nile we have the Libyan Desert, and east of it the so-called Arabian Desert extending to the Red Sea.

North of Cairo the greater part of the country is low-lying, and much of it is of recent origin; but south of that town there is on each side of the river an extensive plateau of Eocene rocks. The western plateau is the larger and more monotonous in character, and extends as far south as

the line of oases of Upper Egypt, where it ends abruptly in a steep escarpment looking southwards. The belt of comparatively low and fertile country in which the oases lie is made of Cretaceous beds, and stretches in a general W.N.W. direction from a point some distance west of Assuan. South of the oases the ground again begins to rise, and forms a second undulating plateau which extends southwards far beyond the boundary of Egypt.

The geological structure on the west side of the Nile is very simple. The formations succeed each other in regular order from south to north, and it is clear that there is a general dip towards the north. But this general simplicity is complicated by various minor anticlinal and synclinal folds, which Lyons (16) has attempted to reduce to a system. The evidence in such a region as the Libyan Desert is necessarily incomplete, but he points out that the principal wells occur along certain lines which run approximately from W.N.W. to E.S.E., nearly parallel in fact to the line of the Upper Egyptian oases. He shows, too, that in one case the line coincides, for a part of its course at least, with an anticlinal axis; and he comes to the conclusion that the other lines also are probably anticlines.

He does not find these lines upon the Eocene plateau, where, indeed, there are no wells; but he believes the Farafra and Baharia oases to be upon an anticlinal axis crossing the others nearly at right angles.

The physical structure of the Arabian Desert is much more complicated. West of a line joining Suez with Kena there is another plateau of Eocene rocks, which is, however, much more cut up by valleys than the Libyan plateau. The most interesting, geologically, of these valleys is the Wadi Araba, which lies between the Northern and Southern Galalas, and opens into the Gulf of Suez about latitude 29° N.

East of this plateau there is a mountain chain which runs nearly parallel to the Red Sea, and consists of a central core of crystalline and schistose rocks, flanked on each side by Cretaceous and later deposits.

The geological formations which are represented in Egypt are not very numerous. For convenience of description they may be grouped as follows :—

Crystalline and schistose rocks.
Nubian Sandstone.
Cretaceous.
Eocene and Oligocene.
Miocene.
Pliocene and recent.

Crystalline and schistose rocks.—The crystalline and schistose rocks which form the basis upon which rest the sedimentary formations of Egypt, are first exposed in the bed of the Nile near the First Cataract at Assuan. Many writers have referred to them more or less briefly, but they have never been very carefully examined. Hawkshaw (13) has given a map of the area, and Dawson (9) has divided the rocks into two series which he correlates with the Laurentian and Arvonian ; while Bonney (6) and Miss Raisin (21) have described their petrological characters. Dykes of diorite appear to be abundant.

According to Floyer (10) the crystalline rocks which form the core of the range of mountains along the shores of the Red Sea, have metamorphosed the sandstone in contact with them ; and according to Johnson (15) there is a similar alteration around the granite near Kalabsheh, above Assuan. Much doubt has been cast on these statements ; but it should be remembered that most large crystalline masses consist of rocks of various ages, and some of these are usually intrusive. Moreover, the "sandstone" which is in contact with the crystalline rocks is probably not everywhere the same age. Some of it is almost certainly Palæozoic and some of it Cretaceous.

Nubian Sandstone.—The "Nubian Sandstone," as the term is generally used in Egypt, includes the whole of the beds which lie between the crystalline rocks and the fossiliferous Cretaceous series. At Assuan it rests upon gneiss and schist ; but a little west of the Nile it extends much further south, and occupies in fact nearly the whole

of the country from the Egyptian oases to Khartum. It rises gradually from beneath the Cretaceous belt of the oases, and forms a barren undulating plateau upon which stand a number of low flat-topped or pyramidal hills.

This, the principal area of Nubian Sandstone in Egypt, extends only a short distance to the east of the Nile; but similar beds underlie the Cretaceous of the Wadi Araba in Middle Egypt.

The characteristic rock of the series is a dark sandstone with a siliceous cement, coloured by iron and manganese. It is often very hard and durable and has yielded much of the stone of which the temples of Nubia and Upper Egypt have been built. But the series is not so homogeneous as its name seems to imply. Lyons (16) describes beds of clay in it, sometimes of considerable extent; and also deposits of iron ore. The latter contain fragments of fossil wood and are believed to have been formed in marshy lagoons.

With the exception of fragments of silicified wood, referred to *Nicolia*, *Araucarioxylon* and other coniferous and dicotyledonous genera, fossils are extremely rare. Several observers have discovered casts of shells, but they are generally indeterminable. Coquand (7), however, has described a specimen from Assuan which he refers to the Upper Cretaceous species *Ostrea Verneuili*.

Until recently this was all the palæontological evidence that we had as to the age of the Nubian Sandstone in Egypt, and it was chiefly because the sandstone is conformably overlaid by the Upper Cretaceous that Zittel (34) considered it to be of Cenomanian age. In the Wadi Araba, however, in beds which were formerly referred to the Nubian Sandstone, Schweinfurth has discovered a fossiliferous band some distance below the Cretaceous; and from this he collected a number of fossils which he at first supposed to be Devonian (25). Further examination (27) has shown the fauna to be a Carboniferous one, and Walther has referred it to the Carboniferous Limestone (32). Among the fossils are such forms as *Productus semireticulatus*, *Streptorhynchus crenis-*

tria, etc., so that the Carboniferous age of the deposit is unquestionable. But Schellwien (22) has recently undertaken a re-examination of the specimens, and comes to the conclusion that they belong to the Upper Carboniferous. He finds two species of *Enteles* [*Enteletes*], a species of *Spirifer* allied to *S. mosquensis*, and other forms characteristic of the Russian facies of the Upper Carboniferous. In fact, according to Schellwien, there is not a single species present which is confined to the Lower Carboniferous, while there are several which have never been found except in the Upper Carboniferous.

However this may be—and the occurrence of *Enteletes* is strong evidence in favour of Schellwien's views—the fact remains undisputed that a part of what was formerly known as the Nubian Sandstone in the Wadi Araba, is of Carboniferous age. The bed in which the fossils were found is succeeded, without any apparent unconformity, by marls and sandstones which are themselves regularly overlaid by undoubted Cretaceous beds with *Exogyra*. Some fifty metres above the Carboniferous fossil bank there is a red sandstone which contains *Araucarioxylon*.

Walther divides the whole of the old "Nubian Sandstone" of the Wadi Araba into three groups :—

1. 100 m. of sandstones and marls which are pre-Carboniferous [or in part Lower Carboniferous].
2. 20 m. of marl and limestone which must be referred to the Carboniferous Limestone [to the Upper Carboniferous].
3. 200 m. of sandstone which may be considered as the equivalents of the Permian, Trias, or Jura.

In Syria and the Sinaitic peninsula also the Cretaceous is underlaid by a sandstone which is generally known as the Nubian Sandstone; and below this at one or two localities there is a limestone band containing Carboniferous fossils (3, 14, 29). The limestone is itself underlaid by another group of sandstones and conglomerates. The sequence, therefore, is precisely the same as in the Wadi Arabi.

By many writers the term Nubian Sandstone is now restricted to the part of the series which lies above the

Carboniferous band, and clearly this must be post-Carboniferous and pre-Cretaceous, or possibly early Cretaceous. But we can say little beyond this. If the apparent absence of unconformity is to be taken as evidence, the whole of the period between the Carboniferous and the Upper Cretaceous must be represented; and this is scarcely probable.

There is nothing to show which part of the Wadi Araba sequence corresponds with the Nubian Sandstone of the Libyan Desert, excepting only the occurrence of *Araucarioxylon*—a genus of great vertical range. Lyons remarks that he is unable to distinguish any divisions in the sandstone of the Libyan Desert, and refers the whole of it to the Cenomanian. But in so extensive and so imperfectly examined an area, negative evidence can go for little.

Cretaceous.—The Cretaceous rocks form the floor of the fertile belt in Upper Egypt, which includes the Farafra, Dakhla, Kharga, and Kurkur oases. In the case of the Dakhla oasis it also forms the northern wall. The Baharia oasis, which lies in a depression in the Eocene plateau, is also cut down to the Cretaceous rocks below. South of the Kharga oasis there are one or two outliers resting upon the Nubian Sandstone. Bir Murr, for example, is stated by Lyons (16) to lie in Cretaceous rocks; and the Selima oasis, S.W. of Wadi Halfa, is also believed by him to be Cretaceous.

About a dozen miles west of the great pyramids there is a rhomboidal area of Cretaceous rocks, which has been brought into its present position among the Tertiary deposits by faults along its four sides. This area has been described by Mayer-Eymar (17), Schweinfurth (28), and Walther.

On the east side of the Nile the Cretaceous forms a belt which lies along the western flank of the mountain chain near the shores of the Red Sea. It has also been found in patches on the eastern flank. It occurs, too, in the Wadi Arabi, and further north still it forms the northern foot of Jebel Attaka, the mountain which is so conspicuous an object from the port of Suez.

The Cretaceous of the Upper Egyptian oases has

been fully described by Zittel (34), who finds that it consists of three main divisions, *viz.* (in descending order)—

3. White well-bedded limestone or earthy chalk.
2. Greenish and ash-grey shaly clays.
1. Beds with *Exogyra Overwegi.*

The *Overwegi* beds, which rest with perfect conformity upon the Nubian Sandstone, are some 130 m. thick. In the lowest part of the series the predominant rock is a brown iron-shot sandstone, in which fish teeth occur in great numbers, and fossil wood is also occasionally found. The teeth belong to the genera *Otodus, Lamna, Corax* (*C. pristodontus*), etc. The middle beds are for the most part variegated clays, with banks of sandstone, marl, and limestone; and it is here that the characteristic fossils of the series, such as *Exogyra Overwegi* and *Inoceramus Cripsi* var. *regularis*, are most abundant. The upper beds, which are chiefly laminated clays, are relatively poor in fossils.

The shaly clays which overlie the *Overwegi* beds are but little different in appearance from those which are found in the *Overwegi* series itself. Besides gypsum and rock-salt they contain also brown iron-ore, and iron pyrites superficially oxidised. Fossils are not abundant except in certain beds, and are fairly distinct from those of the *Overwegi* stage. Among them are *Nautilus danicus, N. desertorum*, etc.

The highest beds of the Cretaceous consist of well-bedded chalky limestone, often somewhat marly at its base. It joins the top of the escarpment which rises on the north side of the Dakhla oasis, while in the Kharga oasis it is itself capped by the Eocene limestones. In the oasis of Farafra it shows all the characters of a true chalk, and consists almost entirely of Coccoliths and Foraminifera. Among the chief fossils which have been found in it are *Ananchytes ovata, Gryphea cf. vesicularis, Pecten Farafrensis*, etc., and numerous fish teeth.

Many of the species which occur in the Egyptian Cretaceous are new; but the presence in the lower beds,

of the very characteristic *Exogyra Overwegi*, suggests that the whole series must belong to the Upper Senonian, for in Tripoli *Exogyra Overwegi* is associated with *Ostrea larva*. Others of the common fossils, such as *Corax pristodontus*, *Nautilus danicus*, etc., are also Senonian forms, and the whole fossiliferous Cretaceous of Upper Egypt is correlated by Zittel with the Upper Senonian or Danian. This view is further supported by the fact that there is no break between the chalky beds at the top of the series and the nummulitic rocks above.

The floor of the Baharia oasis, which lies some distance north of the great belt of oases, is formed of sandstones, marls, etc. They present a considerable resemblance to the lower part of the *Overwegi* stage, but are succeeded by nummulitic limestone at so short an interval as to preclude the possibility of the presence of the higher Cretaceous beds except in a very attenuated form; and hence Zittel was forced to the conclusion that they must be of Tertiary age. Lyons, however, has since discovered *Exogyra Overwegi* in these beds, and thus shown them to be Cretaceous (16).

As yet there is no proof on the west side of the Nile of the existence of any Cretaceous rocks older than the Senonian. But in the Arabian Desert east of that river the Senonian is underlaid by other beds containing a rich and varied Cenomanian fauna.

Near the Monastery of St. Paul, south of the Wadi Araba, Schweinfurth finds a reddish-brown sandstone resting directly upon the crystalline rocks. This is succeeded by an alternation of marly and calcareous beds in which *Hemiaster cubicus* occurs in profusion. Other common fossils are *Sphærulites Schweinfurthi*, *Exogyra Africana*, *Plicatula Fourneli*, *Ammonites Mantelli*, etc., and the whole assemblage can be matched in the Cenomanian deposits of Algeria, Calabria, and other Mediterranean districts.

In some localities these beds are immediately succeeded by a white chalky limestone; but in others, for instance, at the Monastery of Saint Antony in the Wadi Araba, there is an intervening series of variegated marls with inter-

bedded limestones, which contain *Ostrea larva*, *O. serrata*, *Gryphea vesicularis*, etc., and are clearly of Senonian age.

Further north, at the foot of Jebel Attaka, Vaillant (31) long ago discovered a series of Cretaceous rocks. They consist chiefly of limestones, generally dolomitic, and in these he discovered two fossiliferous bands. The lower contained *Hippurites cornu-vaccinum* and *H. organisans* and the upper included *Ostrea larva*, *Janira sexangularis*, etc. The former is very probably Turonian and the latter Senonian. Here, as elsewhere in Egypt, the Cretaceous is succeeded conformably by the Eocene.

Eocene and Oligocene.—The Eocene is by far the most widely spread formation in Egypt. On the west side of the Nile it forms the extensive plateau which reaches from the oases of Upper Egypt northwards as far as Cairo and the Great Petrified Forest. In the Arabian Desert it occupies most of the space between the valley of the Nile and the crystalline core of the Red Sea Mountains; the eastern boundary nearly coinciding with a line joining Suez and Kena (23).

The formation is divided by Zittel into three stages:—

Upper Eocene stage.
Mokattam stage.
Libyan stage.

The Libyan stage forms the southern part of the great Eocene plateau and crowns the escarpment which overlooks the Farafra and Kharga oases. Lithologically there is no definite line between it and the Cretaceous below; but the fossils are very different. The Libyan beds contain *Operculina libyca* and nummulites, which in this region have never been found along with Cretaceous fossils.

The older division of the Libyan stage is well seen in the southern escarpment of the Eocene plateau, and also in the valley of the Nile between Esna and Thebes. It consists chiefly of calcareous rocks, the lowest beds being sometimes marly or sandy. The most characteristic fossil is *Operculina libyca*, which sometimes fills whole banks. Various nummulites also occur, such as *N. Ramondi*, *N.*

Guettardi, *N. Biarritzensis*. Gasteropods and lamellibranchs are abundant, and sea-urchins of various kinds. More than two-thirds of the species are said by Zittel to be limited to the Libyan beds; but there are sufficient which are found elsewhere to determine the geological age with some accuracy. The three nummulites mentioned occur in the Lower Eocene of Europe and India.

The upper division of the Libyan stage is typically developed in the valley of the Nile between Siut and Cairo; and on the right bank opposite Minieh it is found to consist of a succession of beds of limestone. It has also been described by Fraas (11) at one or two points between Cairo and Suez. The most characteristic fossils are Alveolinas of various species (*A. oblonga*, *A. frumentiformis*, etc.). *Fabularia Zitteli* is also abundant; and near Cairo the rock is in places nearly filled with the claws of *Callianassa*, from which circumstance Fraas bestowed upon the group the name of *Callianassa* bank. Nummulites are not so common as in the lower division.

An analysis of the fossils leads to the conclusion that the upper division of the Libyan is to be correlated with the Londinian (a part of the Lower Eocene), and Mayer-Eymar finds the nearest analogue in the sands of Cuise-la-Mothe.

The Mokattam stage occupies the greater part of the Eocene plateau north of the Baharia oasis; and near the valley of the Nile a tongue of it extends upon the top of the Libyan beds as far south as Siut. It also occurs over a considerable part of the plateau east of the Nile.

But it is in the hill of Mokattam near Cairo that it is best known. Owing to its accessibility this locality has been visited by many geologists, and several of them have published the results of their visits in more or less detail. The earliest satisfactory account is that of Fraas; while in more recent years we have descriptions by Zittel, Schweinfurth (24), Mayer-Eymar (17), and others. Schweinfurth's is by far the most complete and detailed account of the stratigraphy of the hill, and is accompanied by a map and sections in which he shows the distribution of the divisions which he has recognised. Mayer-Eymar has proved the

close correspondence between the Egyptian beds and those of France, and has attempted to correlate even the smaller sub-divisions.

The structure of the hill is very simple. It is formed of a series of beds dipping pretty regularly towards the east. Even at a distance two divisions are clearly recognisable, for the lower beds are white in colour and the upper beds brown. The lower division consists chiefly of fossiliferous limestones characterised by the abundance of nummulites, especially *N. Gizehensis* and *N. curvispira*. Amongst the other fossils we can mention only *Echinolampas Fraasi*, *E. africana*, and the remarkable crab *Lobocarcinus*. An interesting peculiarity of this division is the common occurrence of celestine (1, 4).

The upper division consists of an alternation of brown clays and sands, with beds of limestone. Fossils are not so numerous as in the lower beds, but some banks are filled with *Ostrea Clot-Beyi* and other species of oyster. The large nummulites have mostly disappeared, and *N. Gizehensis* is unknown; but *N. Beaumonti* and *N. sub-Beaumonti* occur throughout.

According to Zittel these divisions are only of local value and cannot be recognised, for example, at Beni Hassan and Minieh. The whole Mokattam stage must, therefore, be looked upon as one, and it is correlated by Zittel with the Parisian. Mayer-Eymar (17), however, recognises at Mokattam all the numerous sub-divisions which he has adopted in the Parisian of Europe, and he states that the resemblance to the European sequence is very close.

In the northern wall of the depression of Fayum a succession of beds very similar to that of Mokattam has been described by Schweinfurth (26). The most interesting of the fossils which he records are the remains of *Zeuglodon* and of a number of Teleostean fish which have been described by Dames. According to Schweinfurth the bed from which they are derived, corresponds with a part of the upper division of the Mokattam stage.

Throughout the greater part of Egypt the marine Eocene concludes with the Mokattam stage. But in the west,

near the oasis of Siwa, Zittel discovered nummulitic beds of a different character. They are only some 10 m. thick, and consist of white and brown limestones, made up almost entirely of fragments of shells. Amongst the fossils which occur in them are *Nummulitis intermedius*, and *N. Fichteli*, which belong to De la Harpe's seventh nummulitic horizon (including uppermost Eocene and lower Oligocene); but along with these are also found such typically Eocene forms as *N. Guettardi*, etc. The sea-urchins belong mostly to species which occur also in the Mokattam stage. The molluscs, according to Mayer-Eymar's determination, indicate a Bartonian age.

North of Mokattam rises the hill which is called Jebel Ahmar. It consists chiefly of a brownish sandstone rendered very tough by a siliceous cement. This is the Jebel Ahmar Sandstone, or Nicolia Sandstone of Zittel, and it rests upon the upper beds of the Mokattam stage. Silicified stems of trees are not uncommon in it, but other fossils are rare. Unger (30), however, has recorded the presence of a *Helix*, and of some almost indeterminable fresh-water shells. More recently Mayer-Eymar has discovered near its base a bed containing fresh-water shells. These he at first referred to the Upper Tongrian (17);[1] but on a closer examination he has found them to belong to the Lower Tongrian (18), and he looks upon the Jebel Ahmar Sandstone itself as Upper Tongrian. The usual view hitherto has been that the sandstone is Miocene; but the evidence for this was very weak.

Whatever the age of the Jebel Ahmar rock may be, it is generally agreed that it is the same as the sandstone which occupies so large a part of the desert west of Cairo. It extends from the Wadi Natrun southwards to about lat. 29° N., and a small area occurs also on the east side of the Nile. In some places the fossil trees are so abundant as to have suggested the name of "Petrified Forests". Along its southern margin it overlies a bed of oysters (*O. Fraasi* and *O. Cloti* [*O. Clot-Beyi*]) of Upper Mokattam age (16);

[1] The Tongrian is one of the divisions of the Oligocene.

but in the Wadi Natrun it is stated by Lyons to rest upon Miocene beds. The palæontological evidence, however, is not given, and these "Miocene" beds may well be Lower Tongrian.

The silicification of the Jebel Ahmar Sandstone and of the trees which it contains, is one of the most interesting problems of Egyptian geology, and has been frequently discussed. Schweinfurth was of opinion that it was due to the former presence of geysers or springs of heated waters, and the discovery of siliceous tubes penetrating the sandstone is strong evidence in favour of this view. Walther has given a figure (33, p. 476) showing these tubes standing up above the weathered surface of the rock. A similar kind of silicification on a large scale is taking place at the present day at the hot wells called Hammam-Meskhutin in the Algerian province of Constantine.

About a dozen miles west of the great pyramids Schweinfurth has found deposits which he referred to the Miocene (28). But they have since been examined by Mayer-Eymar (19), and according to him they belong to the Lower Tongrian. The fossiliferous beds are sometimes covered by a cap of dolerite.

Similarly the supposed Miocene beds on the high ground surrounding the depression of Fayum are believed by Mayer-Eymar to be of Ligurian and Tongrian age. But no satisfactory palæontological data have yet been published concerning these deposits.

Miocene.—The observations of Mayer-Eymar have thrown so much doubt on the supposed occurrences of Miocene rocks in the neighbourhood of Cairo that it has become necessary to exercise some caution in attributing a Miocene age to any of the Egyptian deposits. The difficulty is not lessened by the fact that geologists have used the term in very various senses.

We may, however, without much hesitation, accept as Miocene the rocks which form the walls and most of the floor of the oasis of Siwa (34), and which probably occupy most of the Barka plateau. In the oasis itself they consist of limestones and marls containing numerous echinoderms,

lamellibranchs, and other fossils. These beds are all marine; but south of the oasis, in places resting conformably upon the marine beds, are sandstones and fresh-water limestones with *Physa*, *Planorbis*, and *Limnaea*.

Marine beds containing a similar fauna occupy the greater part of the valleys between Jebel Geneffeh and Jebel Attaka, and between Jebel Attaka and the Northern Galala (5). Fuchs, after a comparison of the fossils from Jebel Geneffeh and from Siwa, has come to the conclusion that they are of an age intermediate between the first and second Mediterranean stages.

Pliocene and recent.—Since the Miocene period the Egyptian area seems to have been subject to comparatively little disturbance; but there has been a gradual elevation of the whole region. At the foot of the Mokattam Hill at Cairo marine sands are found banked up against a vertical cliff of Eocene rocks, which is itself bored by Lithodomus or similar shells (11, 9, etc.). The upper limit of the borings is some 200 feet above the level of the Mediterranean, indicating a rise of land of at least that extent.

Near the pyramids of Giza there is a sand which is characterised especially by the well-known fossil *Clypeaster ægyptiacus* or *pliocenicus*. In the Wadi el Mellaha, some four miles south of the great pyramids there is another bank of sand and sandstone, containing a number of shells of the genera *Strombus*, *Cassis*, *Conus*, etc. According to Mayer-Eymar it is clear that both these deposits lie on the same geological horizon, and from the character of the fauna in the latter he refers them both to the "diluvial" period (17). Neumayr, however, has since shown that they are Pliocene (20).

Still more recent probably than these are the deposits which form the isthmus of Suez. They have been described by Fuchs (12), who comes to the conclusion that, in spite of the great difference between the faunas of the Red Sea and of the Mediterranean, there was a connection between the two seas until at least Pliocene times. He finds nothing but Pliocene or later deposits between Port Said and Suez. Recent deposits with a Mediterranean fauna extend south-

wards beyond the Balah Lakes; from here to Jebel Geneffeh and the middle of the Bitter Lakes, we have recent fluviatile deposits of the Nile; while between these and Suez are recent deposits of the Red Sea. Immediately surrounding the Bitter Lakes there are recent deposits containing an impoverished Red Sea fauna.

Fraas (11) states that Miocene beds are developed at Shaluf (which lies on the Suez Canal some four miles south of the Little Bitter Lake); and this might be supposed to be the remains of an isthmus already existing in Pliocene times. But Fuchs definitely asserts that there is no Miocene present there, or anywhere else along the course of the Canal.

Intrusive and volcanic rocks.—Intrusive dykes of diorite and similar rock appear to be common in the crystalline rocks; but among the sedimentary deposits eruptive rocks of any kind are very rare. Lyons mentions a mass of olivine dolerite intruded into the Nubian Sandstone at Jebel Burka, about 20 m. W.N.W. of Wadi Halfa (16). In the oasis of Baharia a basaltic rock breaks through the Cretaceous Sandstone and forms the caps of some of the hills; but what its relations are to the nummulitic rocks does not appear. It has been examined by Zirkel and found to be an olivine basalt (34). A very similar rock, also an olivine basalt or dolerite, has been described by Arzruni (2) from Abu Zabel on the Ismailia Canal, where it is erupted through Tertiary strata.

In the Wadi Araba a number of small intrusions of basaltic rock have been discovered by Schweinfurth and Walther (32).

Lastly Mayer-Eymar (19) has found a bank of dolerite resting upon Lower Tongrian beds in the hills of the Kum-el-Chachaf. He believes these and the igneous rocks of Baharia and Abu Zabel to have been erupted at the same time, that is to say, at the close of the Lower Tongrian period. If this be so, it is not unlikely that the hot springs which caused the silicification of the Jebel Ahmar Sandstone and of the Petrified Forests, represent the close of the only period of vulcanicity which there has been in Egypt since, perhaps, Archæan times.

Note on "The Geology of the Sahara".—Since the article on the Sahara was written, the fossils collected by Overweg near Bir-el-Hassi, and described by Beyrich, have been subjected to a second critical examination by Frech.[1] The general conclusion that the beds from which they were derived are Devonian remains unaltered.

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PHILIP LAKE.

THE EVOLUTION OF THE THERMOMETER.

THE earliest thermometer appears to have been made towards the close of the sixteenth century, but the name of the actual discoverer is not known. The invention has been variously attributed to Robert Fludd, a London physician and a prolific writer on medicine and alchemy, who died in 1637; to Galileo (prior to 1597); to Cornelius Drebbel of Alcmar (see Boerhaave's *Chemistry*, translated by Shaw, i., 152); to Sanctorius of Padua, and to Father Paul of Cracow, who died about 1591. The balance of evidence appears to rest with Sanctorius, who, in his *Commentaria in Artem Medicinalem Galeni* (p. 736, 1613), claims to have discovered the thermometer. John Alphonsus Borelli (*De Motu Animalium*, ii., 175, 1680), and Malpighi (*Posthumous Works*, Amsterdam, 30, 1697), also give him credit for the invention.

All the earlier thermometers depended upon the expansion of air enclosed in a bulb on the end of a tube, which terminated in an open-topped bulb containing a coloured liquid, or dipped into an open vessel holding the liquid. The expansion or contraction of the air was thus indicated by the movement of the liquid within the tube, but as the change of volume of the air was affected by the atmospheric pressure, and the tube was either not graduated or only marked out in divisions as the taste of the maker dictated, the thermometers were of but little value.

The first alcohol thermometer is said to have been made by Galileo in 1611 or 1612, and the "Florentine thermometers" containing spirit of wine in a sealed tube, were employed about the middle of the seventeenth century, by the Academists del Cimento, and were introduced into England by Boyle. In these thermometers, no attempt appears to have been made to expel air before sealing them, and a lucky find of a number of them in 1829, has shown that their graduation was purely arbitrary. The divisions were duodecimal, each consisting of an enamel bead sealed upon

the tube, and every twelfth division was marked by the use of a larger bead or one of a different colour. Further information as to these thermometers, some of which were sent to the Queen of Poland by the Grand Duke of Tuscany in 1657, and thence went to Paris, has recently been published by M. Maze (*Comptes Rendus*, vol. cxx., p. 732, and vol. cxxi., p. 230).

The difficulty of fixing the zero and the distance between the divisions of the thermometer was early recognised, and the want of uniformity was probably due to the fact that chemists were not aware that the freezing and boiling points of liquids were constant. Hooke, however, in 1665, described in his *Micrographia*, p. 38, a spirit thermometer having the zero at the freezing point of water, which he apparently recognised as constant. Boyle also proposed this standard, recommending the use of distilled water, as he was persuaded that waters of different origin congealed at different temperatures (see *Boyle's Works*, abridged by Shaw, vol. i., p. 579). As an alternative, he proposed the congealing point of oil of aniseed as the zero.

In 1692 Mr. Edmund Halley, Secretary of the Royal Society (see *Phil. Trans.*, 1692-3, vol. xvii., p. 650), published an important paper on the construction of thermometers "for use at any place without adjusting by a standard". He stated that he was not aware of any thermometers which had been graduated except as the workman's fancy dictated, although they were usually graduated from a fixed point such as the freezing point of water, or "the point where the spirit stands when it is so cold as to freeze oil of aniseed". After discussing the relative merits of various liquids, and mentioning that mercury might be most properly used were its expansion more considerable—probably the first mention of mercury thermometers—he recommended spirit of wine as the best.

In 1694 Professor Renaldini of Padua suggested the fixation of unity by the temperature of a mixture of eleven parts of cold water with one part of boiling water, the second degree being obtained by mixing ten parts of cold with two parts of boiling water, and so on. In 1701 New-

ton introduced his linseed-oil thermometer, taking the zero as the temperature of thawing snow, and 12° as that of the blood of a living animal. In a "translation from the Latin"—apparently of a paper by Newton—in the *Philosophical Transactions* for 1701, vol. xxii., p. 824, there is a "table of the degrees of heat," the lower temperatures being presumably taken with the oil thermometer, and the higher by comparison with heated iron. In this table, 0° is the temperature of freezing water, 12° blood heat, and 34° that of strongly boiling water; 192° was the heat of live coals burning in a kitchen fire without the aid of bellows.

A thermometer which is said to have drawn Fahrenheit's attention to the subject of the graduation of thermometers, is said to have been described by M. Amontons in the *Mémoires de l'Academie Royale de Paris* in 1701 or 1702, although the writer has been unable to find the paper in these *Mémoires*. In Amontons' thermometer, by which he is frequently credited with having first shown that the boiling point of water is constant, a long glass tube was turned upwards at the bottom and terminated there in a bulb. The bulb contained air, and the tube contained mercury which formed the indicator. The tube was graduated—on the assumption that the atmospheric pressure is equal to a pressure of twenty-eight inches of mercury—in inches and parts of an inch. The highest point, forty-five inches above the level of the mercury in the bulb, was numbered seventy-three, *i.e.*, the height above the mercury plus the atmospheric pressure. His thermometer was thus extremely long and cumbrous, and was inaccurate owing to no allowance being made for variation of atmospheric pressure.

The Fahrenheit thermometer was first graduated with a scale of 180°, the zero being described as "temperate" and corresponding with 9° of the present centigrade thermometer, 90° being "blood heat," and − 90° being the temperature of a mixture of ice and salt, supposed to be the greatest degree of cold obtainable. In 1714, at the suggestion of Claus Roemer, Fahrenheit changed his zero to the

lowest of these points, taking the freezing point of water as 8° and blood heat as 24°, and finally, about 1720, he divided the degrees into fourths, the freezing point of water then standing at 32° F., blood heat at 96°, and the boiling point of water at 212°. The latter improvement was accompanied by the replacement of spirit of wine by mercury (see *Phil. Trans.*, vol. xxxiii., p. 1, 1724). Although this appears to have been the first true mercury thermometer, the use of that material had been proposed as early as 1692 by Halley, as already mentioned.

In 1724 M. de l'Isle of St. Petersburg introduced a system of graduation in which the boiling point of water formed the zero, and the temperature of the cellars of the Paris Observatory 100°. In 1733 the same observer presented a paper to the Academy of Sciences of St. Petersburg (see also *Phil. Trans.*, vol. xxxix., p. 221, 1736), on a mercurial thermometer, having its zero at the boiling point of water and marked in degrees, each of which represented $\frac{1}{10000}$ part of the total volume of mercury contained in the thermometer.

The original thermometer of René de Reaumur of Rochelle, although far from delicate, was soon largely used. It was introduced before 1730 (see *Mémoires de l'Academie Royale de Paris*, p. 645, 1730), and had a large bulb—as much as three or four inches in diameter—containing a known volume of spirit, the tube being so graduated that each degree represented $\frac{1}{1000}$ part of the total spirit. Ultimately, the system of graduation was altered so that the freezing point of water formed the zero and the boiling point was marked at 80°, the bulb being also reduced in size, but the most important improvement due to Reaumur appears to have been the careful expulsion of air from the spirit and from the thermometer tube before sealing, a precaution which had not previously been taken except in the most primitive way.

In 1740 M. du Crest introduced a spirit thermometer provided with the scales of Reaumur, Fahrenheit, and De l'Isle, and in 1742 the valuable improvement of Anders Celsius was introduced. This thermometer, which was formerly known

as the Swedish and afterwards as the centigrade thermometer, and was not used to any extent in France until after the Revolution, was divided centesimally. The boiling point of water was at first marked as 0° and the freezing point as 100°, but, at the suggestion of Linnæus, this graduation was reversed, the temperature of melting ice being taken as zero and that of boiling water as 100°.

Towards the middle of the seventeenth century, attention began to be directed to methods for measuring higher temperatures than can be shown by ordinary thermometers, and a paper in the *Philosophical Transactions* for 1747 (vol. xl., pp. 397 to 408) stated that Dr. Muschenbroek of Utrecht had invented a "metalline" thermometer, in which the expansion or contraction of rods of metal was caused to show changes of temperature. The instrument, as improved by Dr. Desaguliers, has been described in his *Experimental Philosophy* (vol. i., p. 421, 1734).

The next important improvement in this branch of thermometry was the introduction of the Wedgwood pyrometer (*Phil. Trans.*, vol. lxxii., p. 305, 1782; and vol. lxxiv., p. 358, 1784), in which the high temperatures of a furnace were measured by the diminution in bulk of a block prepared from a pure fire-clay under constant conditions as to the amount of moisture used, and the pressure applied in moulding.

Thermometers for *registering* the highest and lowest temperatures attained during the absence of the operator, were introduced as soon as the ordinary thermometer attained to the dignity of an instrument of precision, and although want of space forbids a description of them, the following brief account, with the sources from which it has been derived, may be of use to those who wish to gain information as to the earlier history of this form of thermometer. J. H. van Swinden (*Dissert. sur la Comp. der Thermom.* (?), p. 253, 1770) described, about 1770, maximum and minimum thermometers made on a plan first contributed by Bernouilli to Leibnitz, and Lord Cavendish, in 1757, described somewhat similar instruments of his own invention (*Phil. Trans.*, vol. l., p. 300). In 1782 the well-

known Six's maximum and minimum thermometers were described (*Phil. Trans.*, vol. lxxii., p. 72), and in 1790 and 1794 respectively, the maximum and minimum thermometers of Rutherford were introduced (*Trans. Roy. Soc. Ed.*, vol. iii., p. 247, 1794). In 1826 Blackadder (*Trans. Roy. Soc. Ed.*, vol. x., p. 337) introduced a registering thermometer in which clockwork was used. It may be mentioned that a seventeenth century thermometer, somewhat resembling that of Six, is in existence among the instruments of the Florentine Academy.

So much for the history of the thermometer as far as the commencement of the present century. Many of the works quoted are not readily accessible, but those bearing on the improvements which have since been introduced may be easily found in our scientific libraries, and the reader may follow the process of evolution from the imperfect instruments before described, to the Baly and Chorley thermometer in which mercury is replaced by a fusible alloy of potassium and sodium for determining temperatures above the boiling point of mercury, and the Le Chatelier pyrometer and the modification of it introduced by Professor Roberts-Austen for the determination of the high temperatures reached in furnaces.

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